Interactions with Context

by

Eric Swanson

B.A., Bard College (1997)
M.A., Yale University (1999)
M.A., Tufts University (2001)

Submitted to the Department of Linguistics and Philosophy
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

August 2006

Author .....
Department of Linguistics and Philosophy
August 16, 2006

Certified by .....
Robert C. Stalnaker
Laurence Rockefeller Professor of Philosophy
Thesis Supervisor

Accepted by .....
Alex Byrne
Professor of Philosophy
Chair of the Committee on Graduate Students

Interactions with Context

by

Eric Swanson

Submitted to the Department of Linguistics and Philosophy
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy.

August 16, 2006

Abstract:

My dissertation asks how we affect conversational context and how it affects us when we participate in any conversation—including philosophical conversations.

Chapter 1 argues that speakers make pragmatic presuppositions when they use proper names. I appeal to these presuppositions in giving a treatment of Frege's puzzle that is consistent with the claim that coreferential proper names have the same semantic value. I outline an explanation of the way presupposition carrying expressions in general behave in belief ascriptions, and suggest that substitutivity failure is a special case of this behavior.

Chapter 2 develops a compositional probabilistic semantics for the language of subjective uncertainty, including epistemic adjectives scoped under quantifiers. I argue that we should distinguish sharply between the effects that epistemically hedged statements have on conversational context, and the effects that they have on belief states. I also suggest that epistemically hedged statements are a kind of doxastic advice, and explain how this hypothesis illuminates some otherwise puzzling phenomena.

Chapter 3 argues that ordinary causal talk is deeply sensitive to conversational context. The principle that I formulate to characterize that context sensitivity explains at least some of the oddness of 'systematic causal overdetermination,' and explains why some putative overgenerated causes are never felicitously counted, in conversation, as causes. But the principle also makes metaphysical theorizing about causation rather indirectly constrained by ordinary language judgments.

Thesis Supervisor: Robert C. Stalnaker
Title: Laurence Rockefeller Professor of Philosophy

Contents

Acknowledgements

I am indebted and grateful to many people, and regret that I can mention only a small fraction here. Thanks to my committee: Bob Stalnaker, Steve Yablo, Ned Hall, and Mark Richard. They have taught me so much. Not enough, to be sure, but the fault is mine. I am surprised and humbled whenever I reflect on the ways that they all have helped me. Thanks to Judy Thomson, Nancy Bauer, Vann McGee, Kai von Fintel, Irene Heim, Danny Fox, Jody Azzouni, Jeff King, Dorothy Edgington, Tim Williamson, Alex Byrne, Richard Holton, Rae Langton, William Weaver, and Lindsay Watton. Thanks to all the graduate students of 'Course 24' 2001–2006, and especially to Selim Berker, Tyler Doggett, Andy Egan, David Etlin, Hongwoo Kwon, Bernard Nickel, Dilip Ninan, Chris Robichaud, and Seth Yalcin. Thanks to Sarah Moss for her attention to detail, her patience with broad brush strokes, and her friendship. Thanks to my parents and grandparents for their inspiration as teachers and as people, for their unstinting support, and for their unflagging patience. Finally, profound thanks to Nhu and Liem. I owe them more than could ever be repaid.

CHAPTER 1

Frege's Puzzle and Pragmatic Presupposition

Johnny Ramone is John Cummings. And yet an assertion of (1) may not convey any new information about Johnny Ramone to an addressee $\mathcal{A}$, whereas an assertion of (2) may well give $\mathcal{A}$ new information about Johnny Ramone.

(1) Johnny Ramone is in the Ramones.

(2) John Cummings is in the Ramones.

How do (1) and (2) convey different information? Call this **the question of informativeness**.

It was once common to answer this question in a very straightforward way. Many philosophers held that some coreferential proper names, like 'Johnny Ramone' and 'John Cummings,' have different semantic values. If this were right then (1) and (2) would also have different semantic values, because some of their parts would make different contributions to the truth conditions of the whole. The difference in informativeness between (1) and (2) could then be chalked up to a difference in their semantics. But arguments by Kripke, Putnam, and others convinced many that the semantic value of a proper name *just is* its referent. 'Johnny Ramone' and 'John Cummings' have the same referent, and so on this line they have the same semantic value. But then (1) and

(2) have the same semantic value, ruling out a straightforward semantic answer to the question of informativeness.

In light of this dialectic many philosophers of language have argued there are *pragmatic* differences between sentences like (1) and (2) that are not accompanied by semantic differences.[1] I think they are right on both counts. But I also think that these philosophers have not located the right pragmatic differences. I argue here that (1) and (2) carry different pragmatic presuppositions. This difference between them lets us answer the question of informativeness while holding that coreferential proper names have the same semantic value.

One might think that it does not much matter, from a philosophical point of view, *which* pragmatic answer we give to the question of informativeness. But it does matter, quite a bit, because how we answer the question of informativeness makes a difference to how we should explain the differences between (3) and (4).

(3) Sal believes that Johnny Ramone is in the Ramones.

(4) Sal believes that John Cummings is in the Ramones.

And it's plausible that understanding the semantics and pragmatics of belief ascriptions will bring some insight into the nature of belief itself. My presuppositional answer to the question of informativeness suggests that to better understand proper names' behavior in belief ascriptions we should carefully examine the general behavior of presupposition-carrying expressions in belief ascriptions. This behavior in turn suggests that the value of the context variable that helps determine linguistic expressions' semantic values can shift in the scope of an attitude verb. This sort of context variable shifting would also explain our judgments about (3) and (4), and would do so consistently with the thesis that names that are coreferential in a context have the same semantic value in that context. So my presuppositional answer to the question of informativeness, together with facts about the general behavior of presupposition-carrying expressions in belief ascriptions, suggests that we should pursue a 'presupposition shifting' treatment of sentences like (3) and (4). The opacity effects that we see with proper names are, on my view, a special case of more general phenomena exhibited by presupposition-carrying expressions.

1. See, e.g., Barwise & Perry 1983, 258–264, Salmon 1986, Soames 2002, and Thau 2002, 162–177.

1.1. The presuppositions of proper names

There is a fairly intuitive sense in which proper names are *somehow* associated with *some* content over and above their referents. But it is not easy to say just what this content is, and just how it is associated with proper names. This section uses standard tests for presupposition to help us answer both these questions. Consider

(5) Fido is hungry.

As we will see, a range of tests suggest that a normal speaker normally won't assertively utter (5) unless she presupposes that the thing she associates with 'Fido' in the context of utterance is the thing the addressee associates with 'Fido' in that context. It's worth pausing for a moment to notice what a modest claim this is: I am in effect trying to show little more than that normal speakers normally presume that they will be understood.

Two notes before we begin. First, even if it is not clear *exactly* what presuppositions speakers make when they assert (5), I need only that *among* them is the presupposition that the speaker and addressee associate the same thing with 'Fido' in the context of utterance. Second, when I say that a sentence presupposes that ϕ, I mean that in normal circumstances a speaker who assertively utters that sentence presupposes that ϕ.

1.1.1. The embedding test

We can distinguish between what (5) asserts and what it presupposes by seeing how the clause behaves when embedded in larger clauses. Consider (6)–(10):

(6) It's not the case that Fido is hungry.

(7) If Fido is hungry, we should feed him.

(8) It's possible that Fido is hungry.

(9) Donna hopes that Fido is hungry.

(10) For all I know, Fido is hungry.

The content of an assertion of (5) does not project when the clause is embedded in sentential contexts like (6)–(10), whereas what the clause presupposes does project. Thus the fact that (6)–(10) do not imply (loosely speaking) that Fido is hungry, while (5) does, shows that (5) asserts but does not presuppose that Fido is hungry. And the

fact that assertions of (5)–(10) all imply (loosely speaking) that the thing the speaker associates with 'Fido' in the context of utterance is the thing the addressee associates with 'Fido' in that context shows that (5)–(10) all carry this presupposition.[2]

1.1.2. The filtering test

Some expressions 'filter' presuppositions. For example, the presuppositions of 'ϕ and ψ' don't include the presuppositions of 'ψ' that are entailed by 'ϕ.' So although (11) presupposes that Sam lost, (12) does not.

(11) Betty knows that Sam lost.

(12) Sam and Tim both lost, and Betty knows that Sam lost.

Now consider (13):

(13) This dog is named 'Fido,' and Fido is hungry.

Intuitively, a speaker who uses (13) does not presuppose that she and her addressee associate the same thing with 'Fido' in the context of utterance. That is why she begins by saying "This dog is named 'Fido' "—she thereby *ensures* that she and her addressee associate the same thing with 'Fido.'

It's also worth noting the differences between (13) and (14).

(14) #Fido is hungry, and this dog is named 'Fido.'

(14) is a decidedly odd way to inform someone who might not know which dog the speaker associates with 'Fido' that Fido is hungry. It is odd because the first conjunct of (14) presupposes that the speaker and addressee associate the same thing with 'Fido' in the context of utterance, and then the second conjunct goes on to ensure that (inter alia) that presupposition is satisfied.

The antecedents of conditionals also filter presuppositions. For example, (15) presupposes that Harry is married, but (16) does not:

(15) I'm sure that Harry's wife is a good cook.

(16) If he's married, then I'm sure that Harry's wife is a good cook.

2. If conventional implicatures are effectively "scopeless," (Potts 2005, 28, 35, and passim) then they will pass this embedding test, too. In section 1.4 I argue on independent grounds that the presuppositions conveyed by proper names are not conventional implicatures.

Likewise, (17) presupposes that the speaker and addressee associate the same thing with 'Fido,' but (18) does not.

(17) Fido belongs to the Joneses.

(18) If this dog is named 'Fido' and that dog is named 'Spot,' then Fido belongs to the Joneses.

1.1.3. The 'Hey, wait a minute' test

Addressees can felicitously echo a presupposition of (19) with (20).

(19) Max quit smoking.

(20) Hey, wait a minute! I had no idea Max smoked.

But they cannot felicitously echo what's been asserted in the same way:

(21) #Hey, wait a minute! I had no idea Max quit smoking.

When it's generally felicitous to echo that ϕ in this way, the echoed utterance generally conveys the presupposition that ϕ.[3]

The 'Hey, wait a minute' test works as stated only when

1. The addressee is warranted in complaining that the speaker has presupposed something that isn't mutually presupposed; and

2. The addressee is able to accommodate that presupposition.

In many cases involving proper names, it is so easy to accommodate the relevant presupposition that it would be pedantic for the addressee to complain that the speaker had tried to 'put something over' on her. For example, if exactly one dog is salient, an addressee may be able to figure out which dog the speaker associates with 'Fido.' In such cases Condition 1 usually won't be met. And there are many other cases in which it is impossible for the addressee to accommodate the presupposition that she and the speaker associate the same thing with the relevant proper name in that context, because the addressee doesn't associate anything with the name, or doesn't associate just one thing with the name, or doesn't know what the speaker associates with the name. In such cases Condition 2 won't be met.

3. For this version of the test, see Shanon 1976 and von Fintel 2004.

The standard 'Hey, wait a minute' test is not a very good guide to the presuppositions carried by proper names, because with names often either Condition 1 or Condition 2 is not met. But note that if an addressee doesn't know which thing the speaker means to refer to with 'Fido'—say, there are ten similarly salient dogs in the room—she may well respond to an utterance of (5) with

(22) Hey, wait a minute! Which dog is named 'Fido'?

This response is appropriate precisely because Condition 2 is not met. A natural response for the original speaker would be to point at one dog and say

(23) Sorry; *this* dog is the one named 'Fido.'

The speaker's assertion of (23) ensures, inter alia, that the addressee knows that they associate the same thing with 'Fido' in the context of utterance. It thereby makes the relevant presupposition of (5) appropriate.

1.2. Semantics and pragmatic presupposition

The fact that proper names carry presuppositions might be taken to constrain our theorizing about their semantics. In particular, it might seem that directly referential expressions cannot convey presuppositions. In this spirit, Bart Geurts writes that "For a Millian a name has no meaning, no descriptive content that could steer us towards its referent" (1997, 204; see also his 2002). Geurts is right that there is no descriptive content in the semantics of proper names, if they are directly referential. But we should not overlook the possibility that utterances can communicate descriptive content by non-semantic means.

Note that proper names are not the only expressions at stake here. For example, the nominal complement in complex demonstratives ('dog' in 'that dog') is a presupposition carrier (Glanzberg & Siegel 2006). Simple demonstratives also carry presuppositions: 'this' conveys that its referent is relatively proximal, 'that' conveys that its referent is relatively distal, and both words convey that there is a mutually manifest referent that can be referred to with a singular term. Likewise, pronouns carry presuppositions corresponding to their phi-features—gender, number, animacy, person, and so on (Heim 1982, Cooper 1983). And a case can be made that even core indexicals like 'here' and 'now' convey presuppositions. If all this is right, then *all* putative directly referential expressions carry presuppositions, and to deny that directly referential ex-

pressions can carry presuppositions is in effect to deny that there are any directly referential expressions. But this is the wrong conclusion to draw. The fact that an expression has non-semantic descriptive content simply doesn't bear on the question of whether it is directly referential.

I can say more, however, by explaining how proper names convey the presuppositions they do. The explanation I'll offer is compatible with any semantics on which coreferential proper names have the same semantic value. In particular, it is compatible with the thesis that proper names are directly referential. In giving my explanation I make some assumptions about presupposition and assertion that I will leave undefended here. First, I assume that presupposition should be analyzed as a propositional attitude had by the participants in a conversation. As Stalnaker puts it,

> To presuppose something is to take it for granted, or at least to act as if one takes it for granted, as background information—as *common ground* among the participants in the conversation . . . [Presupposition] is a social or public attitude: one presupposes that ϕ only if one presupposes that others presuppose it as well. (2002, 701)

Common ground has an iterative structure: the proposition that ϕ is common ground in a conversation iff all participants in the conversation treat it as true for purposes of that conversation, all believe that all treat it as true for purposes of that conversation, all believe that all believe that all treat it as true, and so on. Second, I assume that one effect of successful assertion is to update the common ground. That is, successful assertions "... change the presuppositions of the participants in the conversation by adding the content of what is asserted to what is presupposed" (Stalnaker 1978, 86). Finally, I assume that what this framework and these assumptions formalize is tacitly known by normal, competent language users, and that this is common ground among such language users.

Let's briefly review the problem. Suppose a speaker assertively utters (5):

(5) Fido is hungry.

I have already argued that a normal speaker normally will assert (5) only if she presupposes that the thing she associates with 'Fido' is the thing her addressee associates with 'Fido,' or believes that her assertive utterance of (5) will make this common ground via presupposition accommodation. My task is to explain, in semantically non-committal terms, how this presupposition is conveyed by assertive utterances of (5).

Suppose that the context of utterance and the current context agree on the deno-

tations of 'Fido' and 'is hungry,' and let '$\mathfrak{F}$' denote the proposition that Fido is hungry. On my assumptions, a speaker who asserts (5) aims for $\mathfrak{F}$ to become common ground. For this aim to be realized, the speaker and the other conversational participants must all believe that the speaker's assertive utterance of (5) expressed $\mathfrak{F}$. For them to acquire this shared belief, they must all agree on the denotation of 'Fido' in the context of utterance. And for $\mathfrak{F}$ to become common ground they each must believe that they all treat $\mathfrak{F}$ as true for purposes of conversation, each must believe that they each believe that they each treat $\mathfrak{F}$ as true for purposes of conversation, and so on. So for a speaker's aims in asserting (5) to be realized, it must be or readily become common ground that the conversational participants agree on the denotation of 'Fido' in the context of utterance. Competent language users tacitly know this, and so when a speaker assertively utters (5), her addressee or addressees can infer that she presupposes that they all agree on the denotation of 'Fido.'

Now suppose that at the time of her assertive utterance, the speaker did *not* presuppose that it was or would become common ground that she and her addressee associate the same thing with 'Fido' in the context of utterance. This would mean that, by the speaker's own lights, there was real potential for her assertive utterance to be misunderstood. By her own lights she would have little reason to believe that an assertive utterance of (5) would succeed in making its content common ground. In such circumstances a speaker who wants to make a successful assertion should modify the common ground to minimize the possibility that the sentence she asserts will include constituents that are associated with semantic values in a way that is not common ground. In our example, the speaker should make it common ground which dog she associates with 'Fido,' perhaps by pointing and saying that by 'Fido' she means *that dog*.

This explanation does not involve the semantics of proper names. For this reason it is compatible with any theory on which coreferential proper names have the same semantic value, including theories on which names are devices of direct reference.[4]

4. I am sympathetic with the view that coreferential proper names have the same semantic value, but are type $\langle e, t\rangle$ predicates routinely supplemented by phonologically unrealized determiners. This view makes it easy to explain the relationships between determiners and proper names that we see in sentences like

   (24) Some Montagues hate the Capulet that our Romeo loves.

   In one sense of 'directly referential,' on this view proper names are not directly referential. But of course this view needs a pragmatic answer to the question of informativeness, too. (For a range of views according to which proper names are predicates, see Burge 1973,

1.3. Answering the question of informativeness

We can now explain how assertions of (1) and (2) can differ in informativeness.

(1) Johnny Ramone is in the Ramones.

(2) John Cummings is in the Ramones.

Consider an addressee for whom (1) is uninformative and (2) is informative. In normal circumstances such an addressee will take it to be common ground that the thing she associates with 'Johnny Ramone' is the thing the speaker associates with 'Johnny Ramone.' She will similarly take it to be common ground that the thing she associates with 'John Cummings' is the thing the speaker associates with 'John Cummings.' For this reason she will take a speaker who asserts (1) to be trying to communicate information about the man the addressee associates with 'Johnny Ramone,' and a speaker who asserts (2) to be trying to communicate information about the man the addressee associates with 'John Cummings.'[5] Thus the relevant information that she will glean from an assertion of (2) is that the man she associates with 'John Cummings' is in the Ramones. And though our believer does know that the man she associates with 'Johnny Ramone' is in the Ramones, she does not know that the man she associates with 'John Cummings' is in the Ramones. This is why (2) is informative to her, while (1) is not. Note that this difference in informativeness does not depend on the speaker's being similarly ignorant about the 'true identity' of Johnny Ramone and John Cummings: all it takes is for the addressee to fail to presuppose that Johnny Ramone is John Cummings. And because the information that the addressee acquires is information about the person that the speaker intends to be talking about, the addressee will not have misunderstood the speaker—though of course she might have understood him better.

It's true I haven't characterized the addressee's presuppositions in much detail. But the question that is my concern here is about how the use of language can affect the cognitive states of believers who are already in tremendously complex cognitive states.

Hornsby 1976, Larson & Segal 1995, 161–223, and Elugardo 2002.)

5. If the addressee did *not* take it to be common ground that they associate the same thing with 'John Cummings,' she would not take the speaker to be trying to communicate about the man the addressee associates with 'John Cummings.' For more on the role of common attitudes in communication, see Grice 1957, 219–221, Grice 1987, 30–31, Lewis 1969, Schiffer 1972, 137–155, Stalnaker 1978, Clark & Marshall 1981, Clark & Carlson 1981, Clark et al. 1983, and Stalnaker 2002.

It has nothing to do with the characterization of those cognitive states themselves. My explanation does appeal to the possibility of presupposing that Johnny Ramone is not John Cummings, though Johnny Ramone is John Cummings. And as we will later see, this sort of attitude is crucial to my treatment of belief ascriptions. But I think no one should deny that this sort of pragmatic presupposition is possible. Different theorists may analyze attitudes like this in different ways, of course, but we can answer the question of informativeness without having a settled view on which of those ways is right. In light of this I want to emphasize that I am *not* advocating a 'hybrid' view on which the name 'Johnny Ramone' is directly referential but has an associated Fregean concept (Heck 1995). We do need more than a few tidy singular propositions to characterize the doxastic state of a believer who presupposes that Johnny Ramone is not John Cummings. But this doesn't show that a more complicated characterization using possible worlds propositions, Russellian propositions, or some other device would also fail.

We can also now explain the fact that (25) usually will not convey any new information to an addressee, while (26) may impart new information to her.

(25) Johnny Ramone is Johnny Ramone.

(26) John Cummings is Johnny Ramone.

Again, in normal circumstances the speaker will take it to be common ground that he and his addressee associate the same thing with 'Johnny Ramone,' and that they associate the same thing with 'John Cummings.' Likewise for the addressee. A cooperative speaker who uses (26) of course believes that the man he associates with 'John Cummings' is the man he associates with 'Johnny Ramone.' He does not, however, believe that his addressee believes this, or that his addressee believes that the man *she* associates with 'John Cummings' is the man she associates with 'Johnny Ramone.' And so the speaker believes that (26) will be informative to his addressee. The addressee takes the speaker to presuppose that they associate the same man with 'John Cummings' in the context of utterance, and so in interpreting (26) she will take the speaker to be trying to communicate information about the man the addressee associates with 'John Cummings,' to the effect that that man is the man they both associate with 'Johnny Ramone.' The addressee will thus acquire the information that the man she associates with 'John Cummings' is the man she associates with 'Johnny Ramone.'

We can give similar explanations for orthographically indistinguishable proper names, demonstratives, and referential pronouns, because these expressions carry presuppositions relevantly like those of proper names. Suppose we are in a busy harbor,

and our view of *Britannia* is occluded by other ships (Perry 1977). My saying (27) plainly would be uninformative. But you might well find (28) informative.

(27) That [pointing to the bow of *Britannia*] is that [pointing to the same spot].

(28) That [pointing to the bow of *Britannia*] is that [pointing to the stern].

How are we to explain the potential informativeness of (28), on the plausible assumption that it expresses the same proposition as (27)? When a competent speaker uses a demonstrative, she normally presupposes that she and her addressee will take the same thing to be the semantic value of that demonstrative.[6] As this is common ground between the speaker and addressee, an addressee can recover from both (27) and (28) the information that the thing demonstrated when the speaker first said 'that' is the thing demonstrated when the speaker next said 'that.' But we have potential informativeness only with (28), because it is only with (28) that the addressee may not *realize* that the thing demonstrated when the speaker first said 'that' is the thing demonstrated when the speaker next said 'that.'

These explanations are in some ways reminiscent of Stalnaker's 'diagonalization' treatment of identity statements (1978). But even though Stalnaker's account works for sentences like (25)–(28), it's not obvious how to extend the explanation to (1) and (2). This is because diagonalization is a *reinterpretation* strategy, triggered by the assertion of a sentence that would be uninformative if interpreted literally, because the sentence is necessarily true if actually true (1978, 91–92). Contingently true sentences like (1) and (2) generally will not trigger this sort of reinterpretation. The lack of a trigger does not matter for me, because I explain the difference between the information conveyed by (1) and (2) in terms of wholly routine processes of linguistic interpretation. On my account, 'literal' interpretation is not purely semantic—it also involves pragmatic presupposition—and so *re*interpretation simply is not needed to get the right total communicative content.

6. This description of the presupposition could easily be adapted for accounts on which demonstratives are not directly referential (e.g., King 2001). In my 2005, however, I argue that King's case against a directly referential semantics for referentially used demonstratives is inconclusive.

1.4. Communication via pragmatic presupposition

It is uncontroversial that speakers exploit presuppositional phenomena to communicate. For example, (29) can be used to communicate that Ron is married; (30) can be used to communicate that he has kissed her before; and (31) can be used to communicate that the species of bird being demonstrated is *Pelecanus occidentalis*.

(29) Ron's wife is very attractive, too.

(30) You mean he kissed her again?

(31) This *Pelecanus occidentalis* is named Peppy.

Radio sportscasters regularly provide nice examples of this phenomenon, since although they communicate both with each other and with the audience, what is salient to them is often not salient to us. So they use sentences like (32) to guide the radio audience's presupposition accommodation.

(32) I think that giant moth that's been flying around just landed on your shoulder. (Jerry Trupiano to Joe Castiglione, WEEI Radio, Boston, July 10, 2005)

Nevertheless, I am calling on pragmatic presupposition to do much more work than is usual. One might worry, indeed, that some of the effects I have attributed to pragmatic presupposition are in fact realized by some other mechanism. The likely suspects, for someone who holds that coreferential proper names have the same semantic value, are forms of Gricean implicature.

Consider first conversational implicatures.[7] Conversational implicatures generally are cancelable and reinforceable.[8] For example, "Some of the NP-s VP-ed" implicates that not all of the NP-s VP-ed. But this implicature can easily be canceled:

(33) Some of the boys left. But I don't mean to suggest that not all of the boys left—I wouldn't know, since I left early myself.

7. For a view that is like this in some respects, see Salmon 1986, 114–118.

8. See Grice 1987, 39, and Sadock 1978. There are some examples that suggest that cancelability is not a necessary condition for conversational implicatures. But Huitink & Spenader 2004 argues that cancelation resistant conversational implicatures involve "flouting or exploitation of one of the maxims" of conversation. Clearly that's not going on in cases like (35).

And "Some of the boys left" can felicitously be followed with an explicit assertion of this implicature:

(34) Some of the boys left. And, in fact, some of them did not.

The presuppositions of proper names behave very differently:

(35) #Fido is hungry. But I don't mean to suggest that you and I associate the same thing with 'Fido' in this context.

(36) #Fido is hungry. And, in fact, you and I associate the same thing with 'Fido' in this context.

Now consider conventional implicatures. There is little consensus about what conventional implicatures are—if there even are such things.[9] Nevertheless, many have argued that proper names convey conventional implicatures, and have tried to answer the question of informativeness on this basis.[10] So it is important to see how my account differs from that tradition.

'But' is among the best expressions to look to for examples of conventional implicature (Bach 1999, 330). Roughly, the idea is that (37) and (38) share truth conditions, but as a matter of linguistic convention "imply" (loosely speaking) or implicate different things: (37) implicates that there is some contrast between being poor and being honest, and (38) does not (Grice 1961, 127).

(37) She is poor but she is honest.

(38) She is poor and she is honest.

In a very roughly similar way, we might say that (1) and (2) share truth conditions but differ in implicatures. For example, we might say that a speaker implicates with (1) (but not (2)) that her addressee associates the same thing with 'Johnny Ramone' that she does, and implicates with (2) (but not (1)) that her addressee associates the same thing with 'John Cummings' that she does.

(1) Johnny Ramone is in the Ramones.

9. Bach 1999 argues that the canonical examples of conventional implicatures are not implicatures, in virtue of being "part of what is said" (338-339). See also Chierchia & McConnell-Ginet 2000, 353, and Potts 2005, 39–41.

10. See, e.g., Barwise & Perry 1981, Barwise & Perry 1983, 258–264, and Thau 2002, 162–177.

(2) John Cummings is in the Ramones.

By giving a broad enough sense to 'implicate' we can make this suggestion undeniable. But this would not show that proper names do not carry pragmatic presuppositions; rather, it would suggest that on this understanding some (if not all) pragmatic presuppositions also count as implicatures, broadly construed. If we define 'conventional implicature' in this kind of catchall way, I see no reason not to say that the presuppositions conveyed by proper names are also conventional implicatures. But this catchall notion is nearly toothless.

If we define 'conventional implicature' in traditional ways that are less broad, then it is implausible that the content that I say is conveyed by the presuppositions of proper names is in fact conveyed (or is also conveyed) through conventional implicature. This is because conventional implicatures are *commitments* on the part of the speaker.[11] But a speaker is not *committed* to the truth of the content I have been discussing; she need only treat it as true for purposes of conversation. Of course, if the speaker does not believe that the presupposition is true, she will often try to bring her addressee to associate the same thing with 'Johnny Ramone' that she does. (Sometimes, on the other hand, she will happily if a little impolitely presume that her addressee associates the same thing with 'Johnny Ramone' that she does, even if she isn't sure whether this is in fact the case, or believes that it will be the case only after she makes her assertive utterance incorporating 'Johnny Ramone.') But if in the course of a conversation about Johnny Ramone, one participant comes to realize that another has never heard of Johnny Ramone, the first *won't take back anything* that she said or implicated about Johnny Ramone, as she would if she were *committed* to the proposition that the conversational participants associated the same thing with 'Johnny Ramone' in the relevant context. Rather, she will repair the context, by explaining who Johnny Ramone is, and trust that her addressee will then be able to reinterpret her earlier utterances.

Noticing this helps bring out an important intuitive difference between conventional implicatures and presuppositions: presuppositions are (generally) *pre*supposed,

11. For this claim see Grice 1987, 25–26, Bach 1999, 331, and Potts 2005, 11, 33-34. The idea is simply that (37), for example, commits the speaker to there being a contrast—plausibly, a particular kind of contrast—between being poor and being honest.

If I understand him correctly, Scott Soames intends to be neutral on the exact pragmatic mechanism that conveys what he calls the "descriptive content" associated with proper names (2002 and 2005). But Soames does hold that speakers "commit" themselves (2002, 84) to that descriptive content, by "convey[ing] and assert[ing]" it (213).

as content that we must take certain attitudes toward if we are to correctly interpret others' utterances and to correctly predict which utterances others can correctly interpret. By contrast, conventional implicatures, narrowly conceived, are *commitments* on the speaker's part, that are *communicated* by utterances that are correctly interpreted by the addressee. I admit that there is some prima facie tension between this "intuitive difference" between presuppositions and implicatures and my treatment of identity statements, like (25)–(28). But it is not controversial that presuppositional phenomena are *sometimes* exploited to communicate speaker commitments. The crucial difference here is that conventional implicatures *always* communicate commitments. I also recognize that there may be important, neglected differences between phenomena that we currently simply group together under the undifferentiated rubric of 'pragmatic presupposition.' That suspicion is only compounded by the systematicity of the presuppositions carried by proper names, and their backgrounded, non-marked nature relative to more traditional examples of communication via pragmatic presupposition, like (31)–(32). In light of this I want to emphasize that I am not opposed to making finer distinctions amongst presuppositional phenomena than we currently do.[12] But questions about how to classify linguistic phenomena are best answered only when we have *done* a considerable amount of theorizing, because which differences *matter* to us depends in part on which theories we espouse.

1.5. Belief ascriptions

I have already explained how the presuppositions carried by proper names can help us answer the question of informativeness. We should now turn to the challenges posed by belief ascriptions—and in particular, to the fact that there are circumstances and contexts in which we judge sentences like (3) to be true and sentences like (4) to be false.

(3) Sal believes that Johnny Ramone is in the Ramones.

(4) Sal believes that John Cummings is in the Ramones.

(Suppose that Sal believes that Johnny Ramone is in the Ramones, but knows John Cummings only as the husband of Linda Cummings, and so (in one familiar sense)

12. See Goldberg et al. 1990 and Zeevat 1994 for interesting distinctions between kinds of presupposition, and Beaver 2001, 131–133 for some discussion.

does not believe that John Cummings is in the Ramones.) Many reason roughly as follows: our judgments show that (3) and (4) can differ in truth value; so they differ in their semantic value; but they differ only with respect to coreferential proper names; so some coreferential proper names differ in semantic value.

This argument raises hard questions for anyone who denies its conclusion. In thinking about these questions philosophers typically focus on sentences that differ only with respect to coreferential proper names, and even philosophers who consider other sorts of expressions stick to synonyms. Although it's natural to try to build one's theory around those cases that are obviously philosophically interesting, this approach to belief reports has not yet produced much consensus. I suspect that this is because philosophers have been thinking about one manifestation of a more general phenomenon. That is, they have been thinking about the way one kind of presupposition-carrying expression behaves in belief ascriptions, although the behavior exhibited by presupposition-carrying expressions in belief ascriptions *in general* calls for explanation. I argue in this section that a natural explanation of that behavior can be extended to explain the behavior of proper names in belief ascriptions. In effect I am theorizing one or two levels higher in the taxonomy of linguistic phenomena than is usual—at the level of presupposition carriers rather than the level of proper names or of definite noun phrases. If the approach I outline here is successful, it would not only explain the difference between (3) and (4), but would also have greater empirical coverage than many theories that focus exclusively on proper names. So it would thereby unify some otherwise seemingly disparate phenomena.

1.5.1. Local accommodation and the threat of presupposition failure in belief ascriptions

Suppose that Ken is blindfolded, and he is trying to guess who is speaking. We can tell from Ken's guesses that he believes that Louise has spoken once. But we also know that Louise has not spoken—Ken mistakenly thought that someone who sounds like Louise was Louise. That person speaks again, and I say to you

(39) Ken believes that Louise has spoken again.

(39) plainly does not exhibit presupposition failure in this context. But it is not common ground between us that Louise has spoken, and it does not become common ground between us that Louise has spoken. Also note that (39) *would* exhibit presupposition failure if it were not common ground between us that Ken thinks Louise has already

spoken once. This suggests that, even when embedded in the 'that' clause of a belief ascription, 'again' carries presuppositions—presuppositions that in the conversation as described are satisfied by what we take to be Ken's belief state. The example shows that these presuppositions need not be satisfied by the conversational participants' belief states or the conversational common ground.

I want to give a couple more examples to show that this phenomenon is not overly exotic.[13] Suppose we believe and presuppose that there are no spies at the party. But it's also common ground between us that Hob believes there are several. The people that Hob thinks are spies leave, and I say to you

(40) Hob believes that every spy has left.

(40) does not exhibit presupposition failure in this context. But we might expect that it would, because in simple sentences 'every spy' carries the presupposition that it has a non-empty domain, and it's common ground that it has an empty domain. Fortunately, in the conversation described, the presuppositions carried by 'every spy' are satisfied by what we take to be Hob's belief state.

Or suppose we believe and presuppose that Sue has never smoked, but it's also common ground between us that Ron is convinced that Sue does smoke. Then (41) will not exhibit presupposition failure, even though (42) would.

(41) Ron believes that Sue has quit smoking.

(42) Sue has quit smoking.

In (41), the presuppositions carried by 'quit' can be satisfied by what we take to be Ron's belief state; in (42) they would have to be satisfied by the conversational common ground.

It's easy to create more examples like these:

1. Take an expression 'α' that in simple sentences generally carries the presupposition that ψ.
2. Give an example of a conversation in which it is common ground that $\neg\psi$.
3. Consider a non-negated belief ascription that includes 'α' in its 'that' clause, as used in that conversation.

13. See also Stalnaker 1988, 157–158.

4. Notice that the belief ascription carries the presupposition that the ascribee believes that ψ.[14]

To reiterate, in such examples we have a presupposition that cannot be satisfied by the conversational common ground. It *would* be satisfied by what the conversational participants take to be the ascribee's belief state, for purposes of conversation. And in fact, and very broadly speaking, there is a sense in which it *is* so satisfied. I follow Geurts in classifying this phenomenon as a kind of local accommodation (1998, 584–585). To say that an expression is **locally accommodated** in this sense is just to say that some or all of its presuppositions are satisfied by something other than the 'global' or 'basic' conversational context (cf. Heim 1983, 254–255).

We have a choice to make here: we can say either that in cases of local accommodation the complement of the attitude ascription is interpreted relative to a single context that is distinct from the conversational context, or that it is interpreted relative to two or more contexts, at least one of which is distinct from the conversational context. On the one-context approach, although the whole sentence (41) is interpreted relative to two contexts, the complement clause "that Sue has quit smoking" is interpreted relative to a *single* context, as formalized by (43):[15]

(41) Tom believes that Sue has quit smoking.

(43) [Tom believes]^{c_1} [that Sue has quit smoking]^{c_2}

Alternatively, we might say that the complement clauses of attitude ascriptions are interpreted relative to *two* contexts—say, the "basic context" and the "derived context," which is the "set of all possible situations that might, for all the speaker presupposes, be compatible with [the addressee's] beliefs" (Stalnaker 1988, 157).[16] In principle *both* of these contexts are "available to be exploited" in interpreting the complement clause (158). (For convenience I pretend that at most two contexts are ever in play. For example, I call Stalnaker's a 'two-context' approach, although strictly speaking it would be better described as a 'two-or-more-context' approach.)

14. Karttunen claims that '$\mathcal{A}$ believes that ϕ' *always* presupposes that $\mathcal{A}$ believes that ψ, for any presupposition 'ψ' normally carried by 'ϕ' (Karttunen 1973a, 1973b, and 1974; see Heim 1992 for a recent development of the view). Geurts 1998 offers a battery of arguments against this generalization.

15. Heim 1992 takes this kind of approach.

16. See Geurts 1998 for another example of this approach.

Two-context approaches provide a straightforward treatment of sentences like (44), uttered when Tom is not present and it's common ground that the woman demonstrated has never smoked.

(44) Tom believes that that woman has quit smoking.

In particular, we can say that the basic context satisfies the presuppositions of the demonstrative 'that woman,' while the derived context satisfies the presuppositions of 'quit.' But one-context approaches can handle this sort of example, too, as long as they give an appropriate story about the content of c_2. Clearly such approaches cannot simply identify c_2 with Stalnaker's derived context because c_2 does not satisfy the presuppositions of 'that woman.' But c_2 could be the actual conversational context tweaked just enough so that needed presupposition satisfiers can come from what we presuppose to be Tom's beliefs: c_2 could be, as it were, a 'mix' of Stalnaker's basic and derived contexts.[17]

1.5.2. Local accommodation and interpretation in belief ascriptions

If our aim were only to explain how local accommodation in belief ascriptions can allay the threat of presupposition failure, then I suspect that there would be little to decide between one-context and two-context approaches. But on the hypothesis that local accommodation can affect not only whether an expression in a 'that'-clause can be interpreted without presupposition failure but also *how it is interpreted*—given that the basic conversational context ensures that we will *not* have presupposition failure—two-context approaches like Stalnaker's take the day. I think this hypothesis is fruitful and plausible, and I want to give some examples that help show why.

To start consider

(45) Bill believes that the bank manager was rude to him, but she was actually a clerk.[18]

If Mary is the manager and Clara is the clerk, then I think there's a fairly strong intuition that the first clause of (45) does not attribute to Bill the belief that Mary was

17. This is in effect just taking what Heim 1983 says about local accommodation in general (254–255), and applying it to belief ascriptions.

18. As far as I know sentences like this one were first discussed in McCawley 1970. See also Bell 1973 and Hornsby 1977. Recanati discusses phenomena that may be related in many places, including his 1986, 1993, 2000, and 2001.

rude to him, but rather the belief that Clara was rude to him. (Or, for aficionados of 'de re' paraphrases: the belief of Clara that she was rude to him.) For example, a good paraphrase of (45) is:

(46) Bill believes that the person he believes was the bank manager was rude to him, but she was actually a clerk.

This intuition can be explained without appeal to context shifting. If the truth conditions of 'x believes that ϕ' are, roughly, those of 'in every world compatible with x's beliefs, ϕ,' (keeping in mind that the domain for 'every world' may be restricted) then we can render (45) as 'in every world w' compatible with Bill's beliefs, the bank manager in w' was rude to him.' On the assumption that the same person is the bank manager in all of Bill's belief worlds, this gives us the truth conditions of the paraphrase.

But this strategy cannot be generalized very far. For example, it cannot be used with any rigid expression, because given a context of use a rigid expression's associated intension is constant. So on the assumption that proper names are rigid designators, we would still need a story about the relevant reading of sentences like

(47) Jane thinks Tom was rude, but it was actually Todd.

And it's plausible that (47) and (45) should get similar treatments.

If we allow that local accommodation can guide the interpretation of expressions in the complement of an attitude ascription, however, we can say that the interpretation of 'the bank manager' in (45) and 'Tom' in (47) can be guided not by the basic conversational context but by the context that is introduced by local accommodation. So if we presuppose or accommodate the presupposition that Jane thinks Todd is named 'Tom,' we can interpret the occurrence of 'Tom' in (47) as denoting Todd, just as we would interpret an unembedded occurrence of 'Tom' in a context in which all the conversational participants presuppose that Todd is named 'Tom.' Whether 'Tom' is a rigid designator is immaterial, and whether names that are coreferential in a context have the same semantic value in that context is immaterial as well, simply because 'Tom' is associated with different intensions in different contexts.

Setting aside issues about rigidity, the purely intensional strategy also seems unable to explain cases like the following. Conversational context plausibly makes a difference to the intension associated with 'winner' with respect to at least the two dimensions of the class of contestants and the scale of comparison. In non-embedded environments these two dimensions are obviously both determined by the basic con-

versational context. But embedded environments are more complicated. Suppose Steve evaluates cakes 1 through 5, ranking 1 best, 2 next best, and so on to 5, which he says is worst. Unbeknownst to him, a cake contest is going on, and we know that exactly cakes 3, 4, and 5 are the competitors. But of the cakes in the contest, Steve thinks that cake 3 is the best, and we know that the best of the cakes in the contest is the winner. Keeping all this in mind, I think there is a reading on which (48) is true:

(48) Steve believes that cake 3 is the winner.

This suggests that the basic conversational context here determines the class of competitors relevant to the intension of 'winner.' Now suppose that it is common ground that Steve is evaluating the cakes on the basis of how light they are—1 is like gossamer, 5 is rather dense—and it's also common ground that flavor is the only relevant scale of comparison for purposes of the contest. If the basic conversational context also determined the scale of comparison for 'winner,' then (48) would have to attribute to Steve the belief that cake 3 is the best of cakes 3, 4, and 5 *in flavor.* But it has a reading, I think, on which it attributes the belief that cake 3 is the best of the relevant cakes by whatever *Steve's* scale of comparison is. Phenomena like these threaten to crop up for any expression that is sensitive to context in multiple dimensions.

A crude application of the intensional strategy will fail to capture these phenomena, because clearly we cannot analyze (48) as

(49) In every world w' compatible with Steve's beliefs, cake 3 is the winner in w'.

We might, however, treat it as

(50) In every world w' compatible with Steve's beliefs, cake 3 is the winner among the actual class of competitors according to the scale of comparison operative in w'.

But this treatment involves commitment to an surprising amount of syntactic complexity in an expression like 'winner.' Moreover, I think it is quite odd to insist that although conversational context determines the scale of comparison when 'winner' occurs in unembedded environments, it is determined by binding of intensional variables in cases like (48). Finally, if it's common ground that Steve is evaluating on the basis of texture (although he is in fact evaluating on the basis of density) then I think there's a reading of (48) on which it attributes to him the belief that cake 3 is the best with respect to texture. But the intensional treatment cannot explain this reading, because the scale of

comparison, if not determined by the conversational context, can be determined only by Steve's actual beliefs.

We can avoid these problems by appealing to local accommodation. For example, on a one-context view we could say that 'cake 3 is the winner' is interpreted relative to a single context according to which the class of competitors is cakes 3, 4, and 5, and the scale of comparison is lightness. Or, on one two-context view we might say that the class of competitors is determined by the basic context, whereas the scale of comparison is determined by the derived context. Or, because the derived context is not Steve's beliefs simpliciter but rather what the conversational participants *presuppose* to be Steve's beliefs, we might say that the derived context determines both the class of competitors and the scale of comparison. On this line, we treat it as true for purposes of conversation that Steve knows something about the contest (by knowing which cakes are the competitors) without pretending that he knows everything we know about it (since 'winner' can still be evaluated relative to his scale of comparison). The content of that pretense is the derived context. Note that there's nothing remarkable about this derived context: it's easy to imagine basic conversational contexts in which it's presupposed that cakes 3, 4, and 5 are the competitors and the scale of competition is either left an open question or resolved to features that are not in play in the actual competition.

I think these phenomena give us good reason to think that local accommodation can affect not only whether an expression embedded in an attitude ascription is interpretable without presupposition failure, but also how an expression is interpreted, given that the basic context ensures that there will not be presupposition failure. But we have not yet considered any cases that will help us decide between one-context and two-context approaches to local accommodation. The most compelling such cases are those in which it seems plausible that two occurrences of the same expression in an embedded environment get different interpretations. As Stalnaker notes in passing, his two-context approach can "account for Russell's notorious yacht, which may be believed or supposed to be longer than it is" (159).

(51) Speaking of Russell's yacht—Moore believes that it is longer than it is.

Very roughly, the idea is that the two occurrences of 'it' in (51) are interpreted relative to different contexts, and the differences between those contexts—the derived and basic contexts—are such that the two occurrences are interpreted differently. As a result the complement clause as a whole does not express a or the necessarily false proposition. But if, by contrast, the complement clause of (51) is always interpreted relative to a

single context, then *whatever* recipe we give for 'mixing' the basic and derived contexts we will not be able to explain the belief ascription in (51), because both occurrences of 'it' will be interpreted relative to the same context. Similarly for

(52) Pierre doesn't realize that London is London.

By holding that the two occurrences of 'London' have different denotations because they are interpreted relative to relevantly different contexts, we have the beginnings of a story of how (52) can mean that Pierre doesn't realize that some contingent proposition is true. Two-context approaches to local accommodation thus hold out the possibility that they could treat otherwise puzzling sentences like (51) and (52). This sort of treatment is not available on a one-context approach.

It remains to be said exactly how the influence of a non-basic context affects the interpretation of a given expression. In answering this question I think it's helpful to suppose that any expression—'the bank manager,' 'Tom,' 'the winner,' 'Russell's yacht,' 'is longer than,' 'Pierre,' 'realize,' 'London,' or what have you—is associated not only with an intension or intensions, but also with a hyperintension. The hyperintension of an expression is a relation between contexts and intensions (equivalently, a relation between contexts and semantic values). A context bears the hyperintension of an expression to some intension or intensions iff those intensions are associated with the expression in that context. Putting things this way makes it easier to express and appreciate some important facts about the relationship between context, semantics, and linguistic competence. First, because some contexts do not yield an intension for a given expression, an expression's hyperintension need not be defined for every possible context. Second, language users can get by perfectly well without knowing everything there is to know about the hyperintensions of the expressions in their language. Third, much of the knowledge that is relevant to knowing about an expression's hyperintension is both a posteriori and, intuitively, non-linguistic. So while a language user surely must know something about her language's hyperintensions to count as linguistically competent, there is no reason to think that linguistic competence brings with it very much knowledge about hyperintensions. Finally, and crucially, I claim that an expression's hyperintension may relate a single context to more than one semantic value. For example, in a context that does not resolve whether we are talking about color or weight, I claim that 'light suit' has two semantic values. One is the semantic value that it would have in a context that did resolve that we were talking about color, and the other is the semantic value that it would have in a context that resolved that we were

talking about weight. On this view, one way for an expression to be lexically ambiguous on an occasion of use is for the context of use to bear that expression's hyperintension to multiple semantic values.

This approach lets us give a slightly more formal version of the account already sketched for sentences like

(45) Bill believes that the bank manager was rude to him, but she was actually a clerk.

We simply say that the presuppositions of 'the bank manager' are locally accommodated by a context that bears the hyperintension of 'the bank manager' to exactly the clerk Clara. The intuitive gloss on this is that in interpreting (45) we imagine how we would interpret 'the bank manager' if we presupposed of Clara that she was the bank manager. Similarly for

(47) Jane thinks Tom was rude, but it was actually Todd.

Local accommodation here provides a context that bears the hyperintension of 'Tom' to Todd: we interpret 'Tom' as we imagine we would interpret it if we presupposed of Todd that he was named 'Tom.'

It's plausible that the contexts provided by local accommodation in these cases determine a single, actual denotation for the relevant locally accommodated expression. But in more dramatic cases of identity confusion this will not be the case. Consider for example a setting in which (3) and (4) diverge in truth value. (And suppose, again, that in a familiar sense Sal believes that Johnny Ramone is in the Ramones, although he knows John Cummings only as the husband of Linda Cummings.)

(3) Sal believes that Johnny Ramone is in the Ramones.

(4) Sal believes that John Cummings is in the Ramones.

To handle such cases I think we need to say *both* that the hyperintensions of 'Johnny Ramone' and 'John Cummings' take the context provided by local accommodation to multiple referents *and* that those referents are merely possible. The referents of the names—however many referents they have—must be merely possible because it's not true that John Cummings is not Johnny Ramone. And there must be multiple referents because many merely possible people have roughly as good a claim to being the names' would-be referents if Sal's beliefs about who John Cummings is and who Johnny Ramone is had turned out to be true (cf. Quine 1953, 4). So the derived context bears

each name's hyperintension to many merely possible people. As in the simpler cases considered earlier, it's plausible that we use counterfactual reasoning to determine the relevant features of these hyperintensions: we ask how we would interpret 'John Cummings' and 'Johnny Ramone' if the content of our presuppositions were revised to include what we presuppose to be Sal's beliefs about the identities of John Cummings and Johnny Ramone. And we discover that—given Sal's false beliefs about John Cummings 'and' Johnny Ramone—there are many candidate answers to that question that seem equally good. Nevertheless, the propositions that we arrive at for 'John Cummings is in the Ramones' are not among Sal's beliefs—he does not believe that any of the candidate would-be referents of 'John Cummings' are in the Ramones—and I think it's plausible that the propositions we arrive at for 'Johnny Ramone is in the Ramones' do an adequate job of characterizing Sal's belief state given the resources available to the speaker.

I want to draw an analogy that I hope will allay worries about the claim that these belief ascriptions exhibit something like ambiguity. Suppose that in talking to Sal it became clear that he firmly believes that Johnny Ramone is not John Cummings, and suppose he persisted in making claims using the expressions 'Johnny Ramone' and 'John Cummings.' What propositions, *exactly,* would Sal thereby *mean* to express? It is very, very hard to say, because there is something like an ambiguity in his speech. (Which aspects of the actual man does he associate with 'Johnny Ramone,' and which aspects with 'John Cummings'?) The ambiguity that I attribute to the report on Sal's belief state is similar. Of course, for many purposes these ambiguities do not matter, in part because we can work out many of the entailments of Sal's beliefs that do matter—say, that the husband of Linda Cummings is not an influential guitarist, that the guitarist of the Ramones has black hair, and so on.

A natural objection is that this line is inconsistent with the claim that coreferential proper names have the same semantic values. But it's crucial to remember that coreferential proper names are coreferential *relative to a context of use.* There is more than one person named 'John Cummings,' and in some contexts 'John Cummings' will refer to an actual person who is not the famous Johnny Ramone and is not even named 'Johnny Ramone.' So 'Johnny Ramone' and 'John Cummings' are coreferential only relative to a context. A pair of hyperintensions both of which take the actual conversational context to the same referent may take the context introduced by local accommodation to different referents. But there is no inconsistency here, so long as we do not say that 'Johnny Ramone' and 'John Cummings' are coreferential simpliciter, but only relative

to a particular context.

Another natural objection is that actual believers do not stand in any causal or informational relations to merely possible people. Of course I do not deny this. All I am suggesting is that the most efficient way to accurately and informatively *characterize* certain belief states involves mentioning the merely possible.

It's worth noting that local accommodation is not *forced* in the cases we have been discussing in this section, because for the contemplated utterances of (3) and (4) (for example) the content of the actual common ground is sufficient to fix the referents of 'Johnny Ramone' and 'John Cummings.' What, then, would prompt local accommodation of the presuppositions of proper names? We all know from experience that there are often significant differences between the conceptual and linguistic resources of a belief ascriber and those of her ascribee. And when we are asked the philosophically loaded question whether (4) is true if Sal thinks that John Cummings is not Johnny Ramone, these differences are made particularly salient. Consider Mark Richard's remark that

> . . . other than using bribery, threats, hypnosis, or the like, there is simply nothing you can do to get most people to say that Jones believes that Tully was an orator, [even if he believes that Cicero was an orator,] once they know that Jones sincerely denies 'Tully was an orator', understands it, and acts on his denial in ways appropriate thereto. (1990, 125)

In the conversational context that Richard asks us to consider, it is made manifest that there are multiple ways to satisfy the presuppositions carried by 'Tully,' and it is made manifest that what we take to be the ascribee's beliefs satisfy those presuppositions *in a different way* than the conversational context does. In this sort of situation I think it is no great surprise that local accommodation of those presuppositions is preferred. After all, local accommodation of the presuppositions of proper names just makes it more likely that the proposition ascribed will faithfully reflect the ascribee's belief state.

But this is not to say that the presuppositions of a proper name will *always* be locally accommodated when we presuppose that the ascribee believes that the name has a different referent than we do. Even if it's common ground that Glenda knows Bob Dylan only as her childhood friend Robert Zimmerman, if she thinks he has a beautiful voice then in some contexts (53) seems true.

(53) Glenda believes that Bob Dylan has a beautiful voice. (Saul 1998, 366)

I suggest that here we see the globally accommodated reading of 'Bob Dylan,' because

what the speaker is trying to convey with (53) is that Glenda believes that *a voice with the qualities of Dylan's* is beautiful.

Let me briefly recap. My basic strategy has been to assimilate the behavior of proper names in belief ascriptions to the behavior of presupposition-carrying expressions *in general* in belief ascriptions. So I suggested that just as we can interpret 'again' in (39) relative to what we take to be Ken's beliefs, and just as we can interpret 'the bank manager' in (45) relative to what we take to be Bill's beliefs, we can interpret 'John Cummings' in (4) relative to what we take to be Sal's beliefs.

(39) Ken believes that Louise has spoken again.

(45) Bill believes that the bank manager was rude to him, but she was actually a clerk.

Now recall the argument that I sketched earlier about (3) and (4).

(3) Sal believes that Johnny Ramone is in the Ramones.

(4) Sal believes that John Cummings is in the Ramones.

It runs as follows: Our judgments show that (3) and (4) can differ in truth value; so they differ in their semantic value; but they differ only with respect to coreferential proper names; so some coreferential proper names differ in semantic value. I grant that (3) and (4) differ in truth value, and so of course I grant that they differ in semantic value. I also grant that proper names that are coreferential *in a context* have the same semantic value *in that context.* But I deny that the context that is relevant for the interpretation of 'John Cummings is in the Ramones' is the actual conversational context: I hold, indeed, that it is one in which 'John Cummings' and 'Johnny Ramone' are not even coreferential. I think this account is quite plausible once we begin to think of proper names as just another kind of presupposition-carrying expression, thus warranting the treatment we would give to any other presupposition-carrying expression.

CHAPTER 2

The Language of Subjective Uncertainty

In theorizing about linguistic communication it's routine to focus on cases in which one person is certain or nearly certain that ϕ, and would like to convey that ϕ to some other person. I think this focus is misleading. We often communicate from positions of significant subjective uncertainty, and it is not at all obvious what features the 'communication of uncertainties' shares with the communication of certainties or near certainties. This chapter is motivated by the idea that we can learn about communication, and about subjective uncertainty, by learning about the distinctive features of communication *given* significant subjective uncertainty.

Here I will focus on a particular class of expressions that cooperative speakers use to indicate their uncertainty. Consider first some **epistemic modals**:

'It might be that ϕ.'	'It must be that ϕ.'
'Perhaps ϕ.'	'Probably ϕ.'
'It's unlikely that ϕ.'	'It's almost certain that ϕ.'
'It's a little more likely than not that ϕ.'	'Five to one that ϕ.'

Note that epistemic modals exhibit a great *range*: they far outstrip the familiar operators '$\Box$' and '$\Diamond$'. Moreover, epistemic modals can be quite *finely grained*. In some contexts it matters whether we use "Five to one that ϕ" or "Six to one that ϕ" to indicate

our uncertainty with respect to the proposition that ϕ.

We can also indicate subjective uncertainty using what I will call **epistemic adjectives**:

> 'He is a likely F.'
> 'Everyone here is a probable F and a near certain G.'
> 'Few of the people here are both unlikely Fs and possible Gs.'[1]

Although epistemic adjectives resemble epistemic modals in obvious ways, there are a host of compelling reasons to think that epistemic adjectives are not clause-level operators. So a treatment of epistemic adjectives should not treat them *as* epistemic modals at some underlying level of representation.

We can even indicate *relative* subjective uncertainty, using **epistemic comparatives**:

> 'It's likelier that ϕ than that ψ.'
> 'Each one is a possible F and a possible G, but more likely an F than a G.'
> 'However likely it is that ϕ, it's every bit as likely that ϕ and ψ.'

One reason why such constructions are interesting is that we can use them to express important relational constraints on credences. For example, they can express (very roughly speaking) that one's conditional probability of ψ on ϕ is near 1, or greater than 0.5, or what have you.

This chapter starts with some challenges for truth-conditional analyses of epistemically hedged sentences. My aim is not to establish that such sentences lack truth conditions altogether: when I say that they lack *substantive* truth conditions I mean just that *if* they have truth conditions, then those truth conditions do not give their meaning.[2] I present the challenges I do to sharpen a quite general point. It seems mistaken (to many) to characterize doxastic states that involve significant subjective uncertainty purely in terms of propositional content, and it also seems mistaken (to many) to characterize "uncertain evidence" purely in terms of propositional content (Jeffrey 1968, 36). Similarly, I suggest, we should not aim to characterize the *language* of subjective uncertainty in purely propositional terms. To do so would be in effect to seek a function $f(\cdot)$ from degrees of uncertainty and propositions into propositions, such that a believer is uncertain to degree n about a given proposition just in case she is certain about the proposition that is the image of that degree/proposition pair under $f(\cdot)$. This

1. See Huddleston & Pullum 2002, 557–558 for more examples of epistemic adjectives.

2. Lewis 1970, 220–226 takes this sort of view on imperatives.

task looks quixotic if not impossible. My alternative is to use probabilistic tools to represent the 'content' of the language of uncertainty—just as we use probabilistic tools to represent subjective uncertainty and uncertain evidence.

In particular, my semantics associates with a declarative sentence not a proposition—a function from possible worlds into truth values—but rather a set of functions from propositions into values in the interval $[0, 1]$. Those values represent degrees of belief, and those sets of functions are constraints that the speaker advises her addressee to conform her belief state to. My semantics is compositional and integrable into familiar type-theoretic compositional semantic theories. Moreover, unlike standard 'force modifier' approaches, it gives us a plausible analysis of epistemic adjectives. In particular it gives us a plausible analysis of their behavior under quantifiers.

To the extent that this treatment of the language of subjective uncertainty is successful, it's natural to ask whether we can also think of the language of 'subjective certainty' as a way of conveying doxastic advice. I think we can, and probably should.[3] But such a shift in perspective requires us to rethink the nature and norms of assertion. For example, although propositions represent ways the world might be, in general a set of functions from propositions into $[0, 1]$ does no such thing. So I cannot say that to assert that ϕ is to represent the actual world as being a world in which ϕ.[4] A fortiori I cannot simply appeal to the norms that govern representing the world as a world in which ϕ to explain the norms that govern asserting that ϕ. What I say, rather, is that some credences are more accurate estimates of truth value than others, and that considerations about doxastic accuracy contribute to making some assertions—qua pieces of doxastic advice—better than others.[5]

The move to a probabilistic semantics also raises interesting questions at the interface between semantics and context change. It's standard to think of pragmatic presupposition as an attitude that does not come in degrees—it simply rules in or out

3. An interesting question that I do not address here is whether my focus on *doxastic* advice is too narrow. For example, perhaps some normative language could be treated as advice about (say) what sort of value function to have. Note that one could have such a view and still be a realist about the *quality* of advice, and about which value functions are better than others.

4. Compare Stalnaker's first "truism" about assertion: "an act of assertion is, among other things, the expression of a proposition—something that represents the world as being a certain way" (1978, 78).

5. For more on thinking about credences as estimates of truth value, see Jeffrey 1986 and Joyce 1998, 587.

certain possibilities. I think this presumption is entirely right. When I use a definite noun phrase like 'Ron's wife' I *simply presuppose* that Ron is married.[6] The relationship between semantic value and context change potential is complicated, then, by the fact that credence is degreed although conversational presupposition is not. But these complications are in fact welcome, because epistemically hedged statements can affect conversational context in ways that are significantly different from statements that are not epistemically hedged: non-hedged assertions typically exclude possibilities from the conversational context set, but 'might' statements (among other epistemically hedged statements) typically ensure that the context set *includes* the relevant possibilities.

2.1. Challenges for truth-conditional analyses

Why should we think that epistemically hedged sentences—sentences of the form 'It might be that ϕ,' for example—lack substantive truth conditions? This section raises a number of challenges for truth-conditional analyses of epistemic modals in particular. It will be obvious that particular truth-conditional analyses can meet *some* of the challenges I raise. What is wanting is a truth-conditional analysis that meets *all* these challenges. I also want to emphasize that my aim is not to close the door on truth-conditional analyses altogether, but simply to provide reasons for us to see whether any non-truth-conditional analyses fare better.

2.1.1. Appropriate use from positions of ignorance

Suppose I have no idea where my car keys are, and neither does my wife. She gets home from work—and so has no good sense of where I've looked—and I ask her if she knows

6. Even if there *were* data suggesting that presupposition comes in degrees, the move to a degreed notion of presupposition would require a complete overhaul of the standard analysis of pragmatic presupposition. I don't see how such an overhaul would go. On the standard analysis, a conversational participant pragmatically presupposes that ϕ just in case she takes it to be common belief that all the conversational participants treat it as true, for purposes of conversation, that ϕ. Analyzing degrees of presupposition in terms of degrees of common belief will not obviously work (though for starting points see the notion of common p-belief developed in Monderer & Samet 1989 and Morris & Shin 1997). Analyzing degrees of presupposition in terms of common belief about degrees of treating as true for purposes of conversation would require, implausibly, that we coordinate not only on the *content* of presuppositions but also on the point-valued *degrees* to which a proposition is presupposed.

where my keys are. She says "They might be on the kitchen table." Now her utterance in this case may or may not be *helpful* to me, because I may have already scoured the kitchen table for my keys. But whether or not her 'might' statement is helpful to me, it is *appropriate,* and she *knows* that it is appropriate. It wouldn't be fair for me to say in response "No, I've already looked on the kitchen table. They're not there. So why did you say they might be there?" All I can say is something like "No, I've already looked on the kitchen table. They're not there."

Truth-conditional semantics for 'might' have trouble making the right predictions about this case. To see why, consider two simple semantics for 'might,' in the vein of Kratzer 1977, 1981, and 1991.

- A *solipsistic* semantics: 'The keys might be on the table' is true iff it's consistent with what the speaker knows that the keys are on the table.

- A *non-solipsistic* semantics: 'The keys might be on the table' is true iff it's consistent with what the speaker and the addressee know, pooled together, that the keys are on the table.

Given standard assumptions about the norms governing assertion, both these semantics wrongly predict that my wife's utterance was inappropriate. According to the solipsistic semantics, she asserted a proposition that (we can suppose) she rightly believed would be uninformative to me: I already knew that she didn't know whether the keys were on the table. But assertions that the speaker believes will be uninformative are generally not appropriate.[7] According to the non-solipsistic semantics, my wife asserted a proposition that concerns not only what *she* knows about the location of the keys, but also what *I* know. Given standard assumptions about the norms governing assertion, on this semantics my wife must be certain (or nearly certain) *that I am uncertain* as to whether the keys are on the table, if her utterance is to be appropriate. And in the situation as described she plainly is not certain about this. This brings out what I hope is a pretty obvious point: a semantics for the language of subjective uncertainty

7. See, for example, Grice 1987, 26. The right formulation of this constraint on appropriate assertion is a delicate matter, since it is appropriate for me to use

(1) Liem, you ate all the cookies.

simply to let my son know that *I* know that he ate all the cookies. But in such a circumstance I do believe that my utterance of (1) will change my son's beliefs and conversational presuppositions. Appropriate uses of 'might' statements do not require this.

is not plausible unless it leaves room for uncertainty about others' epistemic states.[8]

Because these problems are fundamentally about the norms for using epistemically hedged sentences, one might accommodate and thereby defend a particular truth-conditional analysis by rethinking the norms that govern assertions with truth-conditional contents. But we must be cautious with this strategy, because so much of what we think we know about pragmatics depends on what we think we know about the norms of assertion. In particular, it's not at all obvious that we could revise our conception of the relevant norms without having to rethink our theories of conversational implicature from the ground up.

2.1.2. Assessment and epistemic position

It's not controversial that a person's epistemic position can make a difference to how she assesses a given speech act. But assessments of epistemically hedged statements are sensitive to the epistemic position of the assessor in unusual ways. Consider a case like

EAVESDROPPING:

> The White spies are spying on the Red spies, who are spying on the gun for hire. The gun for hire has left evidence suggesting that he is in Zurich, but one clever White spy knows that he is in London. After finding the planted evidence, one Red spy says to the others, "The gun for hire might be in

8. Even apart from treatments like Kratzer's, it's nearly standard to think that a speaker cannot say truly that it might be that ϕ if relevant others know that $\neg\phi$ (Hacking 1967, 146, 148–149; see also Teller 1972, 310–311, DeRose 1991, 586–596, and von Fintel & Gillies 2005, 4–8). But then my wife would take a serious risk of saying something false with her 'might' statement, presumably would know of this risk, and presumably would be criticizable if I knew that the keys weren't on the table.

In light of problems like this, von Fintel and Gillies "guess" that "the proper view is that non-solipsistic *might*-statements are more like conjectures [than like assertions] and as such are not subject to the belief-condition" (2005, 14). I doubt that this is right, because groundless conjectures are inappropriate too. For example, it would be inappropriate for my wife to say (2) or (3):

(2) I conjecture that the keys are on the kitchen table.

(3) I conjecture that it's consistent with what we both know, pooled together, that the keys are on the kitchen table.

> Zurich," and the others respond "That's true." The clever White spy says "That's false—he's in London" to the other White spies, and explains how he knows this.

Many find both the Red spies' utterance of "That's true" and the clever White spy's utterance of "That's false" wholly appropriate. There are a number of different ways we could respond to this judgment,[9] but here I want to use it just to raise a question: What *is* the truth value of "The gun for hire might be in Zurich"? (Suppose, for sake of argument, that this question makes sense.) Truth-conditional analyses of epistemic modals *demand* principled, theory-neutral answers to questions like this one, because without such answers the semantics of epistemic modals is dramatically underdetermined. And I think that few people will be on reflection *sure* that the Red spy's utterance of "The gun for hire might be in Zurich" is clearly true or clearly false. To the extent that we are unsure about our truth-value judgments in cases like EAVESDROPPING, truth-conditional analyses of epistemic modals are dangerously unmoored.

2.1.3. Interaction with wide scope negation

There are important differences between (4) and (5):

(4) John couldn't be in his office.

(5) John isn't in his office.

In general, a speaker who uses (4) presents herself as being less sure that John isn't in his office than she would have if she had used (5). For example, (4) is in many contexts appropriate to use even if the speaker knows only that the lights are off in John's office. But in many such contexts, (5) won't be appropriate; it would be appropriate only if the speaker had 'more direct' evidence that John isn't in his office. Accordingly, an addressee who hears (4) will (ceteris paribus) become less sure that John isn't in his office than she would if she heard (5).

Our analysis of epistemic 'could' is significantly constrained by its behavior in sentences like (4), where it is found under wide scope negation.[10] Suppose, for example,

9. For 'relativist' approaches, see MACFARLANE 2003, EGAN et al. 2005, and EGAN 2005. For an approach that (as I understand it) involves some indeterminacy about 'what was said,' see VON FINTEL & GILLIES 2005, 12–14.

10. For more on wide scope negation over epistemic modals, see VON FINTEL & IATRIDOU 2003, 184; cf. CINQUE 1999, 198.

that we wanted to give truth conditions for 'It could be that ϕ' in terms of consistency between the proposition that ϕ and some as yet unspecified information. That information will either consist wholly of truths, or will include some falsehoods. Let us take each option in turn.

Suppose that (6) is true iff it's consistent with some collection of truths T that John is in his office.

(6) John could be in his office.

Then (4) would *entail* (5) because (4) would be true iff it's inconsistent with a set of *facts* that John is in his office. But this prediction is wrong, because (4) is *weaker* than (5).

Suppose, alternatively, that (6) is true iff it's consistent with some information I—where I includes truths and falsities—that John is in his office. I think that truth-conditional analyses of epistemic modals must take this line to be tenable. But I want to emphasize that there are significant constraints on the story about I:

1. We have to be careful not to overgenerate. Suppose, for example, that for any utterance of 'It must be that ϕ,' the speaker's beliefs are part of the information base I that is relevant to the evaluation of that utterance. Then, counterintuitively, any speaker who believes that ϕ would speak truly in saying 'It must be that ϕ.'

2. I should itself be consistent. With respect to inconsistent I, 'Might ϕ' would be trivially false.

3. The content of I should be relatively accessible to the speaker and the addressee. To the extent that it is not, it will be unclear what proposition was expressed, and as a result it will be unclear how the conversational presuppositions are supposed to be affected by the assertion of the propositions.

It's not legitimate simply to *assume* that these constraints can all be satisfied. The burden is on the defender of truth-conditional analyses to *demonstrate* that they can all be satisfied.

2.1.4. Effects on conversational presuppositions

In just the sense that (4) is weaker than (5), (7) is weaker than (8).

(4) John couldn't be in his office.

(5) John isn't in his office.

(7) It must be raining.

(8) It's raining.

For example, ceteris paribus (7) will make a relevantly uninformed addressee less sure that it is raining than (8) would. But despite this difference in strength, (7) and (8) *both* induce the presupposition that it's raining. So a truth-conditional account needs to capture the subtle difference between the strength of (7) and the strength of (8), while allowing that they both induce the presupposition that it's raining.

Surprisingly, adding wide scope negation yields a marked *difference* in presuppositions:

(9) It doesn't *have* to be raining. (≈ It might not be raining.)

(10) It's not raining.[11]

Appropriately used, (9) makes it *not* presupposed that it is raining, and (10) makes it presupposed that it *isn't* raining. We need an account that respects the common presuppositions of (7) and (8) while allowing for the different presuppositions of (9) and (10). It's not at all clear that a truth-conditional account of epistemic modals can do this while meeting the other constraints I have laid out.

2.2. Writing uncertainty into semantics

The challenges I have presented so far give us reason to consider non-truth-conditional analyses of the language of subjective uncertainty. The approach I pursue here is to make the objects of truth-conditional semantics more fine-grained, in a way that lets them represent the 'content' of the language of uncertainty. I begin by sketching a simple type-theoretic intensional semantics, according to which clauses express propositions—functions from possible worlds into truth values. I then show how to

11. Note that the presuppositions induced by (11) are not the same as those of either (9) or (10).

(11) I'm not saying that it's raining.

For some discussion of sentences like these—examples of 'illocutionary denegation'—see Searle 1969, 32, and Searle & Vanderveken 1985, 4–5.

extend such a semantics so that declarative sentences express the characteristic function of a set of functions from propositions into point values in the interval $[0, 1]$. Both theories are recognizably compositional—indeed, both are *locally* compositional, in the sense of Szabó 2000.

My goal in this section is just to show, in some detail, how a compositional semantic theory can yield such semantic values. It will help, however, to have a rough sense of what *role* these semantic values are supposed to play in a theory of communication. Very roughly: In asserting that ϕ, a speaker advises her addressees to conform her doxastic state to the set of functions that is the semantic value of 'ϕ'. This is admittedly vague, but it should suffice for present purposes. Later I explain the motivation behind this way of thinking about communication, and make it more precise.

I want to emphasize from the outset that some features of the semantic theories I offer here are the result of unforced choices. Moreover, I admit that some of these choices might be shown wrong if I tried to give a semantics for a larger fragment of English. For present purposes I propose that we more or less bracket that possibility. This is reasonable because my aim here is *not* to give a final and unassailable semantics for epistemically hedged English sentences. All I want to do is show where we can *begin* to develop non-truth-conditional, 'probabilistic' semantic theories that are nevertheless compositional.

2.2.1. A semantics of propositions

I will start with a very simple type-theoretic intensional semantics, in the spirit of Lewis 1970, Cresswell 1973, and Montague 1973. This semantics interprets the sentences expressible in a fragment of English by providing a semantic interpretation function ('$[\![\_\_\_]\!]$') from those sentences into propositions.[12]

12. For one example of composition rules that would work or could be easily adapted to work with these types and semantic entries, see Heim & Kratzer 1998, 95. The crucial rules for our purposes are:

Lexical terminals:

If α is a terminal node occupied by a lexical item, then $[\![\alpha]\!]$ is specified in the lexicon.

Functional application:

If α is a branching node and $\{\beta, \gamma\}$ is the set of α's daughters, then α is in the domain of '$[\![\_\_\_]\!]$' if both β and γ are and $[\![\beta]\!]$ is a function whose domain contains $[\![\gamma]\!]$. In particular, $[\![\alpha]\!] = [\![\beta]\!]([\![\gamma]\!])$.

The language $\mathcal{L}$-*prop*

Types:

e is a type (the type of individuals—$D_{\langle e \rangle} = \{$Al, Betty, Clara$\}$);

s is a type (the type of possible worlds—$D_{\langle s \rangle} = W$);

t is a type (the type of truth values—$D_{\langle t \rangle} = \{\textsc{true}, \textsc{false}\}$);

if α and β are types, then $\langle \alpha, \beta \rangle$ (sometimes abbreviated '$\alpha\beta$') is a type;

nothing else is a type.

Semantic entries:

$[\![\textbf{Al}]\!]_{\langle e \rangle} =$ Al

$[\![\textbf{Betty}]\!]_{\langle e \rangle} =$ Betty

$[\![\textbf{Clara}]\!]_{\langle e \rangle} =$ Clara

$$[\![\textbf{is/are tall}]\!]_{\langle e,st \rangle} = \lambda e.\lambda s. \begin{cases} \textsc{true} \text{ if } e \text{ is tall in } s; \\ \textsc{false} \text{ otherwise.} \end{cases}$$

$$[\![\textbf{is/are nice}]\!]_{\langle e,st \rangle} = \lambda e.\lambda s. \begin{cases} \textsc{true} \text{ if } e \text{ is nice in } s; \\ \textsc{false} \text{ otherwise.} \end{cases}$$

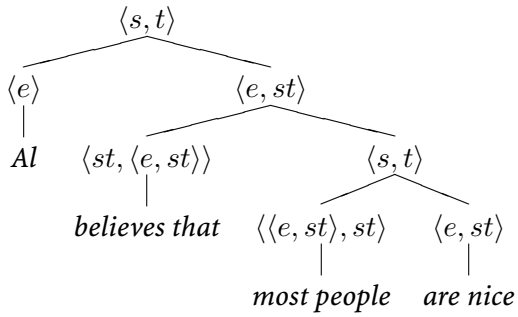
$$[\![\textbf{some person}]\!]_{\langle \langle e,st \rangle,st \rangle} =$$
$$\lambda P_{\langle e,st \rangle}.\lambda s. \begin{cases} \textsc{true} \text{ if some person } e \text{ in } w \text{ is such that } \big(P(e)\big)(s) = \textsc{true}; \\ \textsc{false} \text{ otherwise.} \end{cases}$$

$$[\![\textbf{most people}]\!]_{\langle \langle e,st \rangle,st \rangle} =$$
$$\lambda P_{\langle e,st \rangle}.\lambda s. \begin{cases} \textsc{true} \text{ if most people } e \text{ in } w \text{ are such that } \big(P(e)\big)(s) = \textsc{true}; \\ \textsc{false} \text{ otherwise.} \end{cases}$$

$$[\![\textbf{believe/s that}]\!]_{\langle st,\langle e,st\rangle\rangle} =$$

$$\lambda\phi_{\langle s,t\rangle}.\lambda e.\lambda s.\begin{cases}\textsc{true} \text{ if for every world } s' \text{ compatible with } e\text{'s beliefs in } s,\\ \phi(s') = \textsc{true};\\ \textsc{false} \text{ otherwise.}\end{cases}$$

In ordinary declarative sentences, the semantic values of expressions will combine, via functional application, to yield a proposition. For example, the semantic values of the constituents in "Al believes that most people are nice" combine as follows:



The semantic entries for quantifiers in $\mathcal{L}$-*prop*, though traditional, are the result of an unforced choice. For simplicity consider analogous extensional semantic entries for 'some person' and 'most people':

$$[\![\textbf{some person}]\!]_{\langle et,t\rangle} = \lambda P_{\langle e,t\rangle}.\begin{cases}\textsc{true} \text{ if some person } e \text{ is such that } P(e) = \textsc{true};\\ \textsc{false} \text{ otherwise.}\end{cases}$$

$$[\![\textbf{most people}]\!]_{\langle et,t\rangle} = \lambda P_{\langle e,t\rangle}.\begin{cases}\textsc{true} \text{ if most people } e \text{ are such that } P(e) = \textsc{true};\\ \textsc{false} \text{ otherwise.}\end{cases}$$

On these semantic entries, 'some person' and 'most people' denote the characteristic functions of sets of subsets of the domain of individuals. In particular, those sets are:

$\{X \subseteq \mathbb{D} \mid \text{for some person } e, e \in X\}$

$\{X \subseteq \mathbb{D} \mid \text{for most people } e, e \in X\}$

This sort of approach gives us a relatively intuitive way to think about predication with quantifier phrases: 'Quantifier phrase is an F' is true just in case the set characterized by the semantic value of 'F' is a member of the set characterized by the semantic value of 'Quantifier phrase'.

But we could think about this sort of predication in other ways. For example, we can think of it as a *disjunction* distributed over the members of a set of sets. Let

$$\mathcal{S} = \{X \subseteq \mathbb{D} \mid \text{for some person } e,\ e \in X \text{ and } \forall x \in X, x \text{ is a person}\}$$

and let

$$\mathcal{M} = \{X \subseteq \mathbb{D} \mid \text{for most people } e,\ e \in X \text{ and } \forall x \in X, x \text{ is a person}\}$$

And then consider the semantic entries:

$$[\![\textbf{some person}]\!]_{\langle et,t\rangle} = \lambda P_{\langle e,t\rangle}.\begin{cases}\textsc{true} \text{ if } \bigvee\limits_{X \in \mathcal{S}} \forall x\big(x \in X \supset P(x)\big);\\ \textsc{false} \text{ otherwise.}\end{cases}$$

$$[\![\textbf{most people}]\!]_{\langle et,t\rangle} = \lambda P_{\langle e,t\rangle}.\begin{cases}\textsc{true} \text{ if } \bigvee\limits_{X \in \mathcal{M}} \forall x\big(x \in X \supset P(x)\big);\\ \textsc{false} \text{ otherwise.}\end{cases}$$

This is a longwinded but perfectly workable treatment of quantifiers in subject position. The idea is that one way to say that most Fs are Gs is to say that, for all the sets $1 \ldots n$ in $\mathcal{M}$, all the members of either set 1, or set 2, or set 3, …, or set n are Gs.

We could even imagine a treatment of quantification that returned, as the semantic value of 'Most Fs are Gs,' not a single proposition but *a set of propositions*—in particular, the set consisting of, for all the sets $1 \ldots n$ in $\mathcal{M}$, the proposition that all the members of set 1 are Gs, the proposition that all the members of set 2 are Gs, …, the proposition that all the members of set n are Gs. Such a treatment would be quite indirect, in that the 'content' of an assertion would be recovered by taking the *union* of the sets of which the relevant propositions are the characteristic functions. And surely there is little reason to treat quantification in this way for a language with the expressive power of *$\mathcal{L}$-prop*. But we will soon see why a similar approach is important to giving a

'probabilistic' treatment of certain epistemically hedged sentences.

2.2.2. A semantics of degreed instructions

We can now consider a semantics on which the semantic value of a declarative sentence is the characteristic function of a set of functions from propositions into values in the interval $[0, 1]$. That is, with v as the type of the values in the unit interval, the semantic value of a declarative sentence is of type $\langle\langle st, v\rangle, t\rangle$. Again, think of these semantic values as the content of advice, such that by uttering a declarative sentence a speaker advises her addressees to conform their credences to the functions from propositions to values in $[0, 1]$ that the semantic value of the sentence maps to TRUE.

The language *$\mathcal{L}$-degreed*

Types:

e is a type (the type of individuals—$D_{\langle e\rangle} = \{$Al, Betty, Clara$\}$);
s is a type (the type of possible worlds—$D_{\langle s\rangle} = W$);
t is a type (the type of truth values—$D_{\langle t\rangle} = \{\textsc{true}, \textsc{false}\}$);
v is a type (the type of values in $[0, 1]$—$D_{\langle v\rangle} = [0, 1]$);
if α and β are types, then $\langle\alpha, \beta\rangle$ (sometimes abbreviated '$\alpha\beta$') is a type;
nothing else is a type.

Semantic entries:

$[\![\textbf{Al}]\!]_{\langle e\rangle} =$ Al
$[\![\textbf{Betty}]\!]_{\langle e\rangle} =$ Betty
$[\![\textbf{Clara}]\!]_{\langle e\rangle} =$ Clara

$$[\![\textbf{is/are tall}]\!]_{\langle e,\langle\langle st,v\rangle,t\rangle\rangle} = \lambda e.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if } \mathfrak{P} = \lambda\phi_{\langle s,t\rangle}.\begin{cases}1 \text{ if } \phi = \lambda s.\begin{cases}\textsc{true} \text{ if } e \text{ is tall in } s;\\ \textsc{false} \text{ otherwise.}\end{cases}\\ \textsc{undefined} \text{ otherwise.}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

$$[\![\textbf{is/are nice}]\!]_{\langle e,\langle\langle st,v\rangle,t\rangle\rangle} = \lambda e.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if } \mathfrak{P} = \lambda\phi_{\langle s,t\rangle}.\begin{cases}1 \text{ if } \phi = \lambda s.\begin{cases}\textsc{true} \text{ if } e \text{ is nice in } s;\\ \textsc{false} \text{ otherwise.}\end{cases}\\ \textsc{undefined} \text{ otherwise.}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

Comment: The semantic value of (12) is the characteristic function of the singleton set consisting of the $\langle st, v\rangle$ function that takes the proposition that Betty is nice to 1.

(12) Betty is nice.

$$[\![\textbf{some person}]\!]_{\langle\langle e,\langle\langle st,v\rangle,t\rangle\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$$

$$\lambda P.\lambda\mathfrak{P}.\begin{cases}\textsc{true} \text{ if for some } X \in \mathcal{S}, \text{ for every } x \in X, \mathfrak{P} = \lambda\phi.\begin{cases}v \text{ if } \exists\mathfrak{Q}\Big(\big(P(x)\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\phi) = v\Big)\\ \textsc{undefined} \text{ otherwise;}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

[13]

$[\![\textbf{most people}]\!]_{\langle\langle e,\langle\langle st,v\rangle,t\rangle\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$

$$\lambda P.\lambda\mathfrak{P}.\begin{cases}\textsc{true} \text{ if for some } X\in\mathcal{M}\text{, for every } x\in X\text{, } \mathfrak{P}=\lambda\phi.\begin{cases} v \text{ if } \exists\mathfrak{Q}\Big(\big(P(x)\big)(\mathfrak{Q})=\textsc{true}\wedge\mathfrak{Q}(\phi)=v\Big)\\ \textsc{undefined} \text{ otherwise;}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

Comment: The semantic value of, say, (13) is the characteristic function of the set of $\langle st,v\rangle$ functions that take, for each object in a set X in a set of sets $\mathcal{M}$, the proposition that that person is nice to 1.

(13) Most people are nice.

More concretely, suppose that $\mathcal{M}=\big\{\{a,b\},\{a,c\},\{b,c\},\{a,b,c\}\big\}$. Then the $\langle st,v\rangle$ functions mapped to TRUE by the semantic value of (13) will be the smallest functions such that either

1. the function maps the proposition that a is N to 1, and the proposition that b is N to 1, or
2. the function maps the proposition that a is N to 1, and the proposition that c is N to 1, or
3. the function maps the proposition that b is N to 1, and the proposition that c is N to 1, or
4. the function maps the proposition that a is N to 1, the proposition that b is N to 1, and the proposition that c is N to 1.

13. As before, $\mathcal{S}=\{X\subseteq\mathbb{D}\mid$ for some person e, $e\in X$ and $\forall x\in X, x$ is a person$\}$, and $\mathcal{M}=\{X\subseteq\mathbb{D}\mid$ for most people e, $e\in X$ and $\forall x\in X, x$ is a person$\}$. I am pretending for simplicity that the index of evaluation cannot be shifted—i.e., that 'some person' and 'most people' are always evaluated with respect to the actual world.

$[\![\textbf{believe/s that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle e,\langle\langle st,v\rangle,t\rangle\rangle\rangle} =$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda e.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if } \mathfrak{P} = \lambda\phi.\begin{cases}1 \text{ if } e \text{ is disposed to conform to } \mathbb{P} \text{ in } s;\\ \textsc{undefined} \text{ otherwise.}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

Comment: Many take as a starting point the idea that belief ascriptions of the form '$\mathcal{A}$ believes that ϕ' say that $\mathcal{A}$ stands in the belief relation to the proposition expressed by 'ϕ.' But the idea that 'belief ascriptions' always ascribe full belief, or something close to full belief, is not particularly plausible. Consider

(14) I believe it's unlikely that she agreed.

(15) I believe that perhaps she agreed.

(16) I believe she probably agreed.

(17) I believe she almost certainly agreed.

Intuitively, (14) does not ascribe full (or nearly full) credence in the proposition that it's unlikely that she agreed; (15) does not ascribe full (or nearly full) credence in the proposition that perhaps she agreed; (16) does not ascribe full (or nearly full) credence in the proposition that she probably agreed; and (17) does not ascribe full (or nearly full) credence in the proposition that she almost certainly agreed. Rather, (14) ascribes low credence in the proposition that she agreed; (15) ascribes a somewhat higher (but vaguely adumbrated) level of credence in that proposition; (16) ascribes fairly high credence in that proposition; and (17) ascribes even higher credence.[14]

14. A problem which, as I see it, is a mystery for any extant account, is that in response to (18), (19) seems *more* committal than (20). (Thanks to Ned Hall for this observation.)

$$\Cup_{\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if } \mathfrak{P} = \lambda\phi.\begin{cases}1 \text{ if } \phi = \lambda s.\begin{cases}\textsc{true} \text{ if} \\ \exists\mathfrak{Q}\exists\psi\big(\mathbb{P}(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\psi) = 1 \wedge \psi(s) = \textsc{true}\big) \\ \textsc{false} \text{ otherwise.}\end{cases} \\ \textsc{undefined} \text{ otherwise.}\end{cases} \\ \textsc{false} \text{ otherwise.}\end{cases}$$

Comment: $\Cup$ takes a set of functions that take propositions to 1 and returns the singleton of the function from the union of those propositions to 1. I could have 'written in' this agglomerating effect wherever needed, but introducing a expression to do just this work makes the other semantic entries easier to read.

$$[\![\textbf{it is not the case that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if } \mathfrak{P} = \lambda\phi.\begin{cases}1 \text{ if } \exists\mathfrak{Q}\Big(\big(\Cup(\mathbb{P})\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\bar{\phi}) = 1\Big); \\ \textsc{undefined} \text{ otherwise.}\end{cases} \\ \textsc{false} \text{ otherwise.}\end{cases}$$

(18) Do you know where Mary is?

(19) I think she might be in her office.

(20) She might be in her office.

COMMENT: This entry only handles non-hedged complements. I discuss wide scope negation over epistemic modals later. We need the agglomerating effect of $\Cup$ here because the semantic value of a quantified sentence that is not epistemically hedged is a multi-membered set of functions from propositions to 1.

$[\![\textbf{it might be that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if for some } v \geq \mu, \\ \quad \text{and for the } \phi \text{ and } \mathfrak{Q} \text{ such that } \Big(\big(\Cup(\mathbb{P})\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\phi) = 1\Big), \\ \quad \mathfrak{P} \text{ takes } \phi \text{ to } v \text{ and is otherwise } \textsc{undefined}. \\ \textsc{false} \text{ otherwise.}\end{cases}$$

COMMENT: Think of μ as the least credence that an agent can lend a proposition ϕ and think it might be that ϕ. The value of μ plausibly depends on context, on the nature and importance of the information involved, and so on, but I abstract away from such complications here.

$[\![\textbf{it must be that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if for some } v > (1-\mu), \\ \quad \text{and for the } \phi \text{ and } \mathfrak{Q} \text{ such that } \Big(\big(\Cup(\mathbb{P})\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\phi) = 1\Big), \\ \quad \mathfrak{P} \text{ takes } \phi \text{ to } v \text{ and is otherwise } \textsc{undefined}. \\ \textsc{false} \text{ otherwise.}\end{cases}$$

COMMENT: Note that this semantics secures a sense in which 'might' is the dual of 'must.' Roughly: the semantic value of 'might $\neg\phi$' is the set of functions from the proposition that $\neg\phi$ to values in $[\mu, 1]$, and (given the entry for wide

scope negation over modals that I propose later) the semantic value of '¬ might $\neg\phi$' is the set of functions from the proposition that $\neg\phi$ to values in $[0,1] - [\mu,1] = [0,\mu)$. The probability spaces compatible with that set of functions are exactly those compatible with the set of functions from the proposition that ϕ to values in $(1-\mu,1]$, i.e., those for which $v > (1-\mu)$, as in the entry for 'it must be that' given above.

Also note that there is a clear sense in which 'Must ϕ' is weaker than a non-hedged assertion of 'ϕ'. The former admits assignments of 1 to the proposition that ϕ, but also admits assignments in $(1-\mu,1)$. The latter admits only assignments of 1.[15]

$$[\![\textbf{it can't be that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$$

$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if for some } v < \mu,\\ \quad \text{and for the } \phi \text{ and } \mathfrak{Q} \text{ such that } \Big(\big(\Cup(\mathbb{P})\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\phi) = 1\Big),\\ \quad \mathfrak{P} \text{ takes } \phi \text{ to } v \text{ and is otherwise } \textsc{undefined}.\\ \textsc{false} \text{ otherwise.}\end{cases}$$

15. F. R. Palmer classifies 'must' as a "Deductive" modal, noting that "it is the notion of deduction or inference from known facts that is the essential feature of *must,* not just the confidence of the speaker, which is expressed by the adverbs *certainly, definitely,* etc." (Palmer 2001, 34–35; see also Coates 1983, 41, 131, and 177). I want to emphasize that my aim here is not to *explain why* 'must' and (NB) epistemic 'can't' and 'couldn't' have such an evidential feature; I simply want to provide an account that can accommodate more detailed stories about evidentiality.

$[\![\textbf{it doesn't } \textit{have} \textbf{ to be that}]\!]_{\langle\langle\langle st,v\rangle,t\rangle,\langle\langle st,v\rangle,t\rangle\rangle} =$

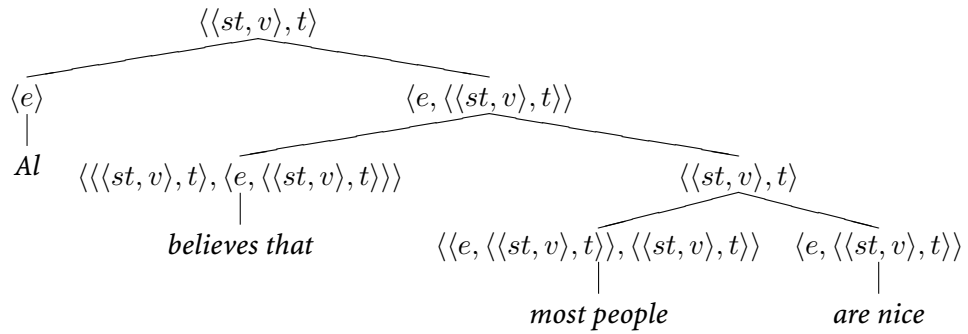
$$\lambda\mathbb{P}_{\langle\langle st,v\rangle,t\rangle}.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if for some } v \leq (1-\mu),\\ \quad \text{and for the } \phi \text{ and } \mathfrak{Q} \text{ such that } \Big(\big(\Cup(\mathbb{P})\big)(\mathfrak{Q}) = \textsc{true} \wedge \mathfrak{Q}(\phi) = 1\Big),\\ \quad \mathfrak{P} \text{ takes } \phi \text{ to } v \text{ and is otherwise } \textsc{undefined}.\\ \textsc{false} \text{ otherwise.}\end{cases}$$

$[\![\textbf{possible}]\!]_{\langle\langle e,\langle\langle st,v\rangle,t\rangle\rangle,\langle e,\langle\langle st,v\rangle,t\rangle\rangle\rangle} =$

$$\lambda P_{\langle e,\langle\langle st,v\rangle,t\rangle\rangle}.\lambda e.\lambda\mathfrak{P}_{\langle st,v\rangle}.\begin{cases}\textsc{true} \text{ if for some } v \geq \mu,\\ \quad \mathfrak{P} = \lambda\phi.\begin{cases}v \text{ if the } \mathfrak{Q} \text{ such that } \big(P(e)\big)(\mathfrak{Q}) = \textsc{true} \text{ maps } \phi \text{ to } 1\\ \textsc{undefined} \text{ otherwise.}\end{cases}\\ \textsc{false} \text{ otherwise.}\end{cases}$$

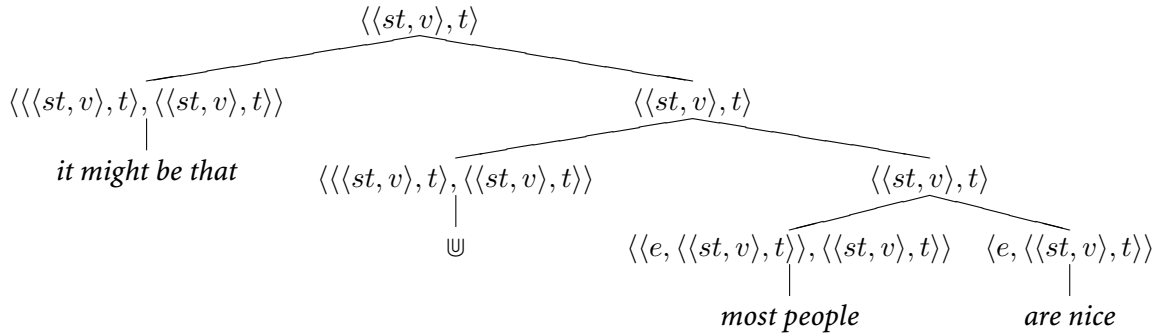
2.2.3. Remarks on $\mathcal{L}$-*degreed*

There are important similarities between $\mathcal{L}$-*degreed* and $\mathcal{L}$-*prop*. At a very abstract level, it is easy to see that the semantic entries of $\mathcal{L}$-*degreed,* like those of $\mathcal{L}$-*prop,* have the right types to combine compositionally:



The fact that the semantic types of *$\mathcal{L}$-degreed* are more *complex* than the types of *$\mathcal{L}$-prop* is no barrier to compositionality.

But *$\mathcal{L}$-degreed* also gives us a compositional, non-truth-conditional treatment of sentences like "It might be that most people are nice":



$\mathcal{L}$-degreed yields as the semantic value of this sentence the characteristic function of the set of functions from the proposition that most people are nice to values in the interval $[\mu, 1]$.

$\mathcal{L}$-degreed does give a non-standard treatment of quantifier phrases. Recall that, according to *$\mathcal{L}$-degreed*, the semantic value of (13) is the characteristic function of the set of $\langle st, v\rangle$ functions that take, for each person in a set X in a set of sets $\mathcal{M}$, the proposition that that person is nice to 1.

(13) Most people are nice.

Clearly it's possible to believe that most people are nice without having any idea *which* people are nice—this is one reason why quantifiers are handy—so it's possible to believe that (20) is true without conforming to any particular *one* of these conditions. Instead think of these conditions as specifying a series of disjuncts, as I suggested earlier. If $\mathcal{M} = \big\{\{a, b\}, \{a, c\}, \{b, c\}, \{a, b, c\}\big\}$, then the whole disjunction is 'Either exactly a and b are N, or exactly a and c are N, or exactly b and c are N, or exactly a and b and c are N.' This disjunction has the same truth conditions as 'Most members of $\{a, b, c\}$ are N,' and so given plausible assumptions a normal addressee will update in the same way in light of either assertion: she will assign high credence to the proposition that most members of $\{a, b, c\}$ are N.[16] Even stronger, a believer whose credences are additive assigns credence v to the proposition that most members of $\{a, b, c\}$ are N iff v is the sum of her credences in the proposition that exactly a and b are N, the proposition that exactly a and c are N, the proposition that exactly b and c are N, and the proposition that exactly a and b and c are N.

'Force modifier' analyses, epistemic adjectives, and quantification

$\mathcal{L}$-degreed has some of the characteristics of 'force modifier' analyses of epistemic modals, according to which epistemic modals indicate "the speaker's assessment of the truth of the proposition expressed in the [sentence's] residue or the nature of the speaker's commitment to its truth" (Huddleston & Pullum 2002, 767).[17] But there are crucial differences between such approaches and mine. This section brings out some of the differences by arguing that although force modifier approaches cannot explain the behavior of epistemic adjectives, my fully compositional theory can. In the course of this I explain why we need *$\mathcal{L}$-degreed*'s nonstandard treatment of quantifiers.

16. For simplicity I ignore the addressee's knowledge that this proposition was expressed in a particular way.

17. For contemporary examples of such views, see Westmoreland 1998, Drubig 2001, von Fintel 2003, and Yalcin 2005. In the end von Fintel does not endorse a force modifier approach.

The guiding idea of force modifier approaches is that in asserting a statement headed by an epistemic modal, a speaker puts forward a *non*-hedged proposition, but with less than the usual authority or certainty. On one such view, for example, epistemic modals are "modulators of assertive force" (Yalcin 2005, 18). To my knowledge no advocates of force modifier approaches have tried to extend their theories to cover epistemic adjectives. But they must, at some point: epistemic modals and epistemic adjectives are equally a part of the language of subjective uncertainty, and they are interesting for many of the same reasons. If a theory of epistemic modals in particular cannot be generalized to constitute a theory of the language of subjective uncertainty, then that is a weighty consideration against it.

First consider

(21) Al is a possible hire.

The only plausible force modifier treatment of (21) that I can see gives it the logical form of (22).

(22) [It's possible that [Al is a hire]]

From a purely syntactic point of view it would be better (ceteris paribus) to say that 'possible' combines with 'hire' to form the predicate 'possible hire':

(23) [Al [is a possible [hire]]]

$\mathcal{L}$-degreed treats 'possible' in just this way. Granted, according to it (21) has the same semantic value as (22). But the *route* to that semantic value is very different. *$\mathcal{L}$-degreed* analyzes 'possible' as a predicate modifier that takes the semantic value of 'hire'—a function from an object to (the characteristic function of) a (singleton) set of functions from the proposition that that object is a hire to 1—and yields as the semantic value of 'possible hire' a function from an object to (the characteristic function of) a set of functions from the proposition that that object is a hire to the values in $[\mu, 1]$.

Let me give another example, which makes it even clearer that (ceteris paribus) we should avoid syntactically revisionary theories of epistemic adjectives. Consider (24), which is epistemically hedged in two different ways.

(24) Al is a likely candidate and a possible hire.

In normal circumstances, an addressee's belief state after interpreting (24) will be no different (modulo beliefs about the mode of expression) than it would have been if the

speaker had said

(25) It's likely that Al is a candidate. It's possible that Al is a hire.

This suggests, plausibly enough, that in some sense (24) and (25) have the same or very similar *content*. But it is quite another thing to say that they have the same underlying syntactic structure. I see no way for force modifier views to avoid this implausible commitment. By contrast, *$\mathcal{L}$-degreed* treats epistemic adjectives as adjectives: there is nothing surprising going on in the syntax.

I think these considerations are compelling, but I admit that others may not, or may think that this bit of syntactic revisionism is acceptable. The fact that quantifiers can scope over epistemic adjectives is stronger reason to think that force modifier approaches are on the wrong track.[18] Consider (29):

(29) This is an easy job; the person we hire for it doesn't need any special qualifications. So even though only one person will be hired for the job, most of the applicants are possible hires.

The speaker here says that only one person will be hired for the job—thus *denying* that it's possible that most of the applicants are hires—and yet consistently with that says that most of the applicants are *possible* hires. So

(30) Most of the applicants are possible hires.

has a reading on which the quantifier scopes over the epistemic adjective.

It turns out to be surprisingly difficult to give a formal characterization of what this reading means, so long as we are supposing that (30) does not simply express a proposi-

18. Von Fintel & Iatridou 2003 argues at length for a "descriptive generalization" to the effect that "A quantifier cannot have scope over an epistemic modal" (174). I think this generalization admits of exceptions:

(26) Al might be the best candidate, Betty might be the best candidate, and Clara might be the best candidate. So everybody here might be the best candidate. (EVERYBODY HERE $>$ $\Diamond$)

(27) Most people here could be the best candidate. (MOST PEOPLE $>$ $\Diamond$)

(28) No one here *has* to be the murderer. (NO ONE $>$ $\Box$)

At any rate, von Fintel and Iatridou take no stand on whether quantifiers can scope over epistemic adjectives, and I think that (30) clearly exhibits such a scope relation.

tion put forward with *ordinary* assertive force. This is because there is *no* proposition, put forward with *whatever* force, that gives the meaning of the relevant reading of (30). The proposition that at least one applicant is a hire is clearly too weak, and the proposition that most applicants are hires is too strong (again, put forward with whatever force) because a speaker who says (30) does not thereby commit herself to lending non-zero credence to more than one applicant being a hire.[19] This shows that epistemically hedged statements cannot in general be analyzed as ways of putting forward a non-hedged proposition with less than the usual certainty, force, or authority.[20] Of course, one person's modus ponens is another's modus tollens. We *could* take all this to suggest that (30) *does* express an ordinary proposition—say, the Kratzerian proposition that most of the applicants have the property of a particular epistemic community's not knowing them not to be hires. But I think that taking the result in this way would be premature, because when integrated into an appropriate theory of the role the semantic value of a declarative sentence plays in communication, *$\mathcal{L}$-degreed* gives us a successful treatment of (30).

The semantic value of (30), according to *$\mathcal{L}$-degreed,* is the characteristic function of a set of type $\langle st, v\rangle$ functions. Those $\langle st, v\rangle$ functions are the smallest functions that take, for each person in a set X in a set of sets $\mathcal{M}$, the proposition that that person is a hire to a value in $[\mu, 1]$. More concretely, suppose that $\mathcal{M} = \big\{\{a, b\}, \{a, c\}, \{b, c\}, \{a, b, c\}\big\}$. Then the $\langle st, v\rangle$ functions will be those that map

1. the proposition that a is an H to a value in $[\mu, 1]$, and the proposition that b is an H to a value in $[\mu, 1]$, or

2. the proposition that a is an H to a value in $[\mu, 1]$, and the proposition that c is

19. Recall that the proposition that $\bigvee_{X \in \mathcal{M}} \forall x\big(x \in X \supset H(x)\big)$ *just is* the proposition that most of the applicants are hires (letting $\mathcal{M}$ stand for the appropriate set of sets of people).

20. The (as yet obscure) view that quantifiers can 'scope into speech acts' might provide another way to handle epistemic adjectives. (For some work in this vein see Karttunen 1977 and Krifka 2001 and 2004.) But one cannot consistently construe the logical form of (30) as (31) and hold that epistemic hedges simply serve to modify the 'force' associated with the assertion of a single proposition.

    (31) [For most of the applicants]$_i$, it's possible that [that applicant]$_i$ will be the hire.

    So (without a worked-out theory on the table) it is not clear whether we should count such a view as a kind of force modifier view, or as something more akin to my view.

an H to a value in $[\mu, 1]$, or

3. the proposition that b is an H to a value in $[\mu, 1]$, and the proposition that c is an H to a value in $[\mu, 1]$, or

4. the proposition that a is an H to a value in $[\mu, 1]$, the proposition that b is an H to a value in $[\mu, 1]$, and the proposition that c is an H to a value in $[\mu, 1]$.

The most straightforward way for a believer's doxastic state to conform to this sort of set is for it to satisfy one of these constraints directly. But just as one can believe that most people are nice without knowing who is nice, one can believe that most of the applicants are possible hires without having any idea *which* of the applicants are possible hires.

I explained earlier that because the proposition that most people in the set $\{a, b, c\}$ are nice *is* the proposition that exactly a and b are nice, *or* exactly a and c are nice, *or* exactly b and c are nice, *or* exactly a, b, and c are nice, given plausible assumptions to believe that most of the applicants are nice is to believe this disjunction. Similarly, I propose that to believe that most of the people in the set $\{a, b, c\}$ are possible hires is to believe the disjunction "Exactly a and b are possible hires, or exactly a and c are possible hires, or exactly b and c are possible hires, or exactly a, b, and c are possible hires." The disjuncts correspond to the $\langle st, v\rangle$ functions in the set that $\mathcal{L}$*-degreed* yields as the semantic value of 'Most of the people in the set $\{a, b, c\}$ are possible hires.'

Let me make some comments on this proposal. First, it's pretheoretically clear that one can believe that a and b are possible hires without believing that it's possible that a and b are hires. That is—post-theoretically—one can assign a credence in $[\mu, 1]$ to the proposition that a is a hire and assign a credence in $[\mu, 1]$ to the proposition that b is a hire, while assigning a credence in $[0, \mu)$ to the proposition that a and b are both hires. Second, one can believe an ordinary disjunction—one without any epistemic hedges, for example—while having significantly less than full belief in each of its disjuncts. But to *sustain* a belief in an ordinary disjunction a believer must be disposed to update her credences in a way that vindicates certain inferences. For example, a believer who sustains her belief that ϕ or ψ in the face of learning that $\neg\phi$ must come to believe that ψ. Similarly, a believer who sustains her belief that a is a possible hire or b is a possible hire in the face of learning that a is *not* a possible hire must come to believe that b is a possible hire. So a believer who sustains her belief in the disjunction that I associate with 'Most of the people in the set $\{a, b, c\}$ are possible hires' must—to take just one example—come to believe that exactly a and b are possible hires in the face of

learning that c is not a possible hire. This is so despite the fact that one can believe that exactly a and b or exactly a and c or exactly b and c or exactly a, b, and c are possible hires without lending credence μ or above in the proposition that a is a hire, or in the proposition that b is a hire, or the proposition that c is a hire. Put less abstractly: you can consistently believe that most of the people in the set $\{a, b, c\}$ are possible hires without believing that any particular one of them is a possible hire. But if in such a state you learn that c is not a possible hire, and you sustain your belief that most of the people in the set $\{a, b, c\}$ are possible hires, you're rationally constrained to believe that a and b are possible hires.

The success of this explanation depends on my claim that to believe that a is a possible hire a believer must lend at least μ credence to the proposition that a is a hire (where μ is some real number greater than 0, so that there are reals *between* 0 and μ). Indeed, rejecting this claim leads to interesting problems that are not local to this particular explanation. Suppose, for reductio, that we say that to believe that it might be that ϕ it's sufficient that one lend *non-zero* credence to the proposition that ϕ.[21] Then consider a believer who believes that *either* it might be that ϕ or it might be that ψ, without believing that it might be that ϕ and without believing that it might be ψ. Either she assigns non-zero credence to ϕ or she does not, and either she assigns non-zero credence to ψ or she does not. But given our reductio assumption she cannot assign non-zero credence to either without contradicting the stipulation that she does not believe that it might be that ϕ and does not believe that it might be that ψ. So she must assign zero credence to both. This account then does not distinguish between one way of believing that either it might be that ϕ or it might be that ψ and believing that $\neg(\phi \vee \psi)$. Surely there is such a difference. One way to see this difference is to notice that in general a believer who believes that $\neg(\phi \vee \psi)$ and 'learns that $\neg\phi$' will not come to believe that it might be that ψ. Contrast this with a believer who believes that either it might be that ϕ or it might be that ψ, and, in learning that $\neg\phi$, comes to believe that it might be that ψ. These problems bring out how important it is that a believer be able to assign sub-μ credence to a proposition without assigning *zero* credence to that proposition.

21. In his 2005 Seth Yalcin offers a view that is (broadly speaking) in this spirit. Yalcin's aim is admittedly a bit different than mine—he is only trying to characterize changes to conversational context, which need not be the same as changes to doxastic states. But I think Yalcin could not appeal to this distinction in his defense without underscoring the importance of a substantive story about how epistemically hedged statements *do* change and reflect doxastic states.

Adding wide scope negation

The semantic entries I gave for 'it is not the case that,' 'it might be that,' 'it must be that' and so on treat their complements as non-hedged—i.e., as denoting sets of functions from propositions into 1. Among other things, this means that *$\mathcal{L}$-degreed* does not allow one epistemically hedged clause to embed another. The consensus in the current literature seems to be that clauses headed by epistemic modals, at any rate, cannot embed each other, so one might think that this constraint is in order. After all, if we focus on epistemic readings 'It's possible that he might be in the basement' seems to mean the same as 'He might be in the basement' and 'It's possible that he is in the basement.' And 'It must be that he might be in the basement' seems uninterpretable. On the basis of examples like this it's routine to posit collapse of iterated 'harmonic' modals.[22] Roughly, the idea is that epistemic modals of similar 'strength' can be felicitously iterated—and collapse, to their common strength—and those of different strength cannot. I am not sure it's plausible that epistemically hedged clauses *in general* cannot embed other epistemically hedged clauses. That is, once we take an appropriately broad view of the language of subjective uncertainty, including epistemic adjectives, we may find pressures to think that we can embed some epistemically hedged clauses in others. But the issue is complicated enough that I want to leave it for another time. What I want to emphasize is that my choices here are in all important respects not forced. I freely admit that if some epistemically hedged clauses can embed others, *$\mathcal{L}$-degreed* could not capture the meaning of such sentences. But the *basic framework* that I advocate here could easily accommodate a semantics designed to handle the relevant phenomena.

At any rate, because epistemic modals can take wide scope negation the entry for sentential negation given in *$\mathcal{L}$-degreed* leaves a need for an analysis of sentences like (4).

(4) John couldn't be in his office. ($\neg\Diamond\phi$)

That analysis must capture the differences between the meaning of (4) and the meaning of (32).

(32) John might not be in his office. ($\Diamond\neg\phi$)

Notice also that we do not want the 'value range' associated with (4) to be $[0, 1-\mu]$ or

22. On the harmonicity of modals, see Halliday 1970, 331, Coates 1983, 46 and 138, Palmer 2001, 35, and Huddleston & Pullum 2002, 179–180, 182.

$[0, 1 - \mu)$. Rather, the semantic value of (4) should be the (characteristic function of the) set of functions from the proposition that John is in his office to values in $[0, \mu)$. What we would like is a treatment of wide scope negation over epistemic modals that yields this semantic value but does not look ad hoc.

We can give such a treatment by analyzing the negation in (4) as having the relevant features of *constituent* negation, not sentential negation. To see the distinction in an uncontroversial case, consider the difference between

(33) [[[Not many] people] know him].

(34) [[It's not the case that][[[many] people] know him]].

(33) and (34) mean the same thing, or very nearly so.[23] But if we grant that 'not many people' is a syntactic constituent in (33), and that its semantic value is the product of combining the semantic values of 'not' and 'many' and combining the result with the semantic value of 'people,' then we need an account of 'not' as it occurs in this kind of linguistic context. Plausibly, its semantic value is a function from semantic values of quantifier type to semantic values of quantifier type. That is, 'not many' is just as much a determiner as 'many' is, and combines with a predicate like 'people' to form a quantifier.

In particular, this example suggests that when 'not' combines with a determiner to form another determiner, the semantic value of 'not' takes a property of properties P and returns its complement $\bar{P}$. Similarly, I hold that when 'not' combines with an epistemic modal to form another epistemic modal, its semantic value is something like complementation—it takes an $\langle\langle st, v\rangle, t\rangle$ function that effectively assigns a range of values in the unit interval to some proposition, and yields an $\langle\langle st, v\rangle, t\rangle$ function that assigns the *complement range of values* to the same proposition. For example, the range associated with 'it might be that' is $[\mu, 1]$, and the range associated with 'it can't be that' is its complement in the unit interval—$[0, \mu)$. The range associated with 'it must be that' is $(1 - \mu, 1]$, and the range associated with 'it doesn't *have* to be that' is its complement in the unit interval—$[0, 1 - \mu]$. This semantic entry for wide scope negation over epistemic modals gives us the desired results, then. And it does not look ad hoc, insofar as there are clear analogies between this kind of negation and constituent negation in certain quantifiers.

23. For more examples of constituent negation, see Huddleston & Pullum 2002, 431 and 806–812.

Parasitic notions of truth and falsity

I said earlier that the type $\langle\langle st, v\rangle, t\rangle$ functions that this semantics associates with declarative sentences do not aspire to represent anything—they are the content of something like doxastic advice—and so are not assessable for truth and falsity. We can nevertheless define parasitic notions of 'truth' and 'falsity,' if we like, that apply to those $\langle\langle st, v\rangle, t\rangle$ functions that map *only* functions from propositions to 1 to TRUE. Call such functions 'truth-apt.' Call a truth-apt function $\mathbb{P}$ 'true' just in case the function mapped by $\mathbb{U}(\mathbb{P})$ to TRUE maps only true propositions to 1. Otherwise call $\mathbb{P}$ 'false' (abstracting away from presupposition failure and other potential instigators of truth value gaps). This is really just a way of speaking, but it helps bring out an interesting feature of the semantics: An expression that *embeds* a non-truth-apt expression may *itself* be truth-apt. For example, 'It might be that ϕ' is not truth-apt, because some functions that it maps to TRUE take a proposition into a value other than 1. But 'Al believes that it might be that ϕ' *is* truth-apt in this sense. The semantic value of 'believes' is designed to handle both truth-apt and non-truth-apt complements and to invariably yield truth-apt semantic functions. This fact disarms an objection familiar from work on non-truth-conditional theories in other domains, to the effect that if 'ϕ' is not truth-apt, then 'Al believes that ϕ' is, implausibly, also not truth-apt. I can deny this conditional, granting that 'Al believes that ϕ' is truth-apt (in the parasitic sense that I have outlined here) without conceding anything about 'ϕ' itself.

Epistemic comparatives and the type of clauses

According to *$\mathcal{L}$-degreed,* the semantic type of declarative sentences is $\langle\langle st, v\rangle, t\rangle$. But for all I have said so far we could have made them type $\langle st, vt\rangle$. Declarative sentences would then denote functions from propositions into sets of values in the interval $[0, 1]$—more intuitively, functions from propositions into *intervals within* the unit interval. Although this sort of approach would suffice for the sentences we have considered so far, it would not be able to handle comparatives like (35)–(37):

(35) It's likelier that ϕ than that ψ.

(36) However likely it is that ϕ, it's every bit as likely that ϕ and ψ.

(37) Most people here are as likely As and Cs as they are As.

I mention such sentences because I think it is important that our analyses of non-comparative epistemically hedged clauses be integrable into a more general theory that encompasses comparatives. Analyzing declarative sentences as having semantic values of type $\langle\langle st, v\rangle, t\rangle$ makes this possible.

I will not propose a compositional analysis of epistemic comparatives here. But I do want to explain why this semantic type, unlike many others, can give the intuitively right meaning for epistemic comparatives. For simplicity consider just (35). The doxastic states that conform to (35) are exactly those that assign higher credence to the proposition that ϕ than they do to the proposition that ψ. For example, we want to *admit* credence assignments like $\{\langle\phi, 0.9\rangle, \langle\psi, 0.8\rangle\}$, $\{\langle\phi, 0.9\rangle, \langle\psi, 0.7\rangle\}$, and $\{\langle\phi, 0.8\rangle, \langle\psi, 0.1\rangle\}$, and to *prohibit* credence assignments like $\{\langle\phi, 0.9\rangle, \langle\psi, 0.9\rangle\}$, $\{\langle\phi, 0.7\rangle, \langle\psi, 0.8\rangle\}$, and $\{\langle\phi, 0.1\rangle, \langle\psi, 0.8\rangle\}$. $\langle\langle st, v\rangle, t\rangle$ functions can do this. Thinking of them as the characteristic functions of sets of functions from propositions to values, we can say that each member of such a set simply specifies the admissible credence assignments, in line with those just mentioned. $\langle st, vt\rangle$ functions cannot do this: they can specify that the credence in a given proposition must fall in some *range,* but crucially the range itself cannot be specified relative to the credence in another proposition. None of this is to say, of course, that type $\langle\langle st, v\rangle, t\rangle$ is the *only* semantic type that could capture the meaning of epistemic comparatives; obviously type $\langle\langle st, vt\rangle, t\rangle$ would do as well and at the same time add some (likely unneeded) expressive power.

Notice that (36) admits only credence assignments in which the conditional probability of ψ on ϕ is 1.[24] I acknowledge that instances of the sentence schema are a bit of a mouthful. But I find it hard to hear any difference in meaning between (38) and (39):

(38) If he dropped the glass, it broke.

(39) However likely it is that he dropped the glass, it's every bit as likely that he dropped the glass and it broke.

This suggests that, for appropriate 'ϕ' and 'ψ', the meaning of (36) is intuitively the same as that of

(40) If ϕ, then ψ.

I also find it hard to hear any difference in meaning between (41) and (42):

24. If $P(\phi \wedge \psi) \geq P(\phi)$ then $\frac{P(\phi \wedge \psi)}{P(\phi)} \geq 1$, so $P(\psi|\phi) \geq 1$, so $P(\psi|\phi) = 1$.

(41) Most people here are such that if the person is a liar, then he's a crook *and* a liar.

(42) Most people here are as likely crooks *and* liars as they are liars.

These judgments suggest that many—perhaps all—indicative conditionals *are* epistemic comparatives. If all indicative conditionals are epistemic comparatives, then once we have a theory of epistemic comparatives on which they're capable of admitting only credence assignments on which the conditional probability of ψ on ϕ is 1, or at least 0.5, or what have you, we will have a semantics for indicative conditionals *as a special case*.

It's also worth noting that if these hypothesized connections between indicative conditionals and epistemic comparatives hold up under scrutiny, we have a host of reasons to think that at least some epistemic comparatives do not express propositions. For if indicative conditionals do not express propositions, as many think,[25] and the meaning of any indicative conditional just is the meaning of an epistemic comparative, then those epistemic comparatives do not express propositions. Given that we want a uniform treatment of epistemic comparatives, this would suggest that *no* epistemic comparatives express propositions. And if we want a treatment that unifies epistemic comparatives with ordinary epistemically hedged sentences—'It might be raining', 'Most of them are possible hires,' and so on—then we wind up able to draw on every argument that indicative conditionals do not express propositions in arguing that epistemically hedged sentences in general do not express propositions.

2.2.4. Presupposition effects

The move to a probabilistic semantics puts pressure on the influential idea that we describe the doxastic changes associated with a given sentence *in* describing the changes that that sentence makes to the conversational context.[26] Indeed, I think we should

25. Edgington 1995 is an excellent survey.

26. Consider Irene Heim's inspired and inspiring declaration of intent:

> . . . I will suggest that, while the CCP [context change potential] of "if" cannot be derived from its other properties, one *can* derive the content property from the CCP. More generally, the truth-conditional aspect of the meaning of any expression is predictable on the basis of its CCP. (1983, 253)

In later work Heim goes so far as to say that "The meaning of a sentence *is* its context change potential" (1992, 185, emphasis added).

abandon that hypothesis: although there are connections between context change potential (CCP) and 'credence change potential,' neither can supplant the other. Context change potential is too coarsely grained to do the job on its own: 'ϕ' and 'It must be that ϕ' induce the same presuppositions but have different credence change potentials. And credence change potential cannot supplant context change potential for all the reasons usually marshalled to think that truth-conditions cannot do the job. To take a simple example, neither the truth conditions nor the credence change potential of 'ϕ and ψ' determine its CCP.

There are nevertheless some interesting, systematic generalizations to be made about how epistemically hedged statements change conversational context. I'll start by arguing that in a normal conversational context in which no one demurs, an utterance of 'It might be that ϕ' ensures that the conversational participants do not presuppose that $\neg\phi$. I will call this the context change potential of 'might' statements.[27]

Someone who admits that it might be that ϕ may give very little credence to the proposition that ϕ: "I might be a bodiless brain in a vat, but I really doubt it." But despite the low credence given here to the proposition that ϕ, admitting that it might be that ϕ makes it inappropriate to presuppose that $\neg\phi$. Consider this dialogue:

BETTY: I saw Ron walking his dog last night with Sam.

CLARA: Are you sure it was Ron's dog? It might have been a neighbor's.

BETTY: # I think it was Ron's dog, but I might be wrong. Anyhow, Ron's dog was really misbehaving . . .

Betty's response is infelicitous because the presuppositions typically carried by the definite expression 'Ron's dog' are neither in place nor easily accommodated. This phenomenon is explained by my hypothesis about the CCP of 'might' statements. Betty's admission that it might not have been Ron's dog ensures that the context set includes worlds in which Betty was wrong to think that the dog she saw was Ron's dog. And this prevents Betty from appropriately presupposing that 'Ron's dog' denotes the dog she saw. We can now see one reason why it's hard to argue with skeptics: give them an inch of credence, and they are *entitled* to take a mile of presupposition:

RICHARD: My hand hurts.

27. Heim's original way of thinking about context change potentials is significantly less inclusive than this one. She writes, for example, that "There is an intimate connection between the CCP of a sentence and its truth conditional content: . . . To be a true sentence is to keep the context true" (1983, 253).

Tom: Are you sure you have a hand? You might be a bodiless brain in a vat.

Richard: # I think I have a hand, but I might be wrong. Anyhow, my hand has been hurting for several days now.

Richard's response to Tom is not as marked as Betty's response to Clara—but only insofar as Richard is conveying that he'd prefer not to play the skeptic's game today.

Note that these would-be failed presuppositions can be supplied by the antecedent of a conditional, thereby preventing presupposition failure:

Betty: I think it was Ron's dog, but I might be wrong. Anyhow, if it was Ron's dog, his dog was really misbehaving . . .

—

Richard: I think I have a hand, but I might be wrong. Anyhow, if I have a hand, my hand has been hurting for several days now.

The felicity of these responses strongly suggests that what is going on here really is presupposition failure. Given a pragmatic analysis of presupposition, the hypothesized CCP for 'might' statements falls out immediately.

We can see the context-changing effects of 'might' in other places as well. We often use 'might' statements when we reject assertions:

Smith: The weather report says it will definitely rain tomorrow, so it will rain tomorrow.

Jones: It might not rain tomorrow—weather reports are sometimes wrong.

Given a Stalnakerian picture of assertion, the conversational participants have 'taken on board' Smith's assertive utterance that it will rain tomorrow only if the common ground comes to exclude worlds in which it doesn't rain tomorrow, because to assertively utter 'ϕ' is to propose that the common ground exclude worlds in which $\neg\phi$. Jones then exploits the CCP of 'It might not rain tomorrow' to make her rejection of that conversational proposal manifest. Her counterproposal is, in effect, that the common ground *include* some worlds in which it doesn't rain tomorrow. She rejects Smith's assertion by making a proposal that is inconsistent with one of its intended effects.

'Might' statements are often used to structure further inquiry: after someone says that it might be that ϕ, it's often natural to proceed by collectively trying to determine whether the proposition that ϕ is true. (I suspect that this phenomenon can be explained by appeal to the increased common salience of the possibilities raised by

the 'might' statement, together with the operation of Gricean mechanisms that effect a conversational implicature that the speaker does not know whether ϕ and would find it worthwhile to know whether ϕ.) Speakers sometimes exploit this phenomenon by using 'might' statements to make a kind of pseudo-concession. For example, an effective way to respond to and discuss a student's claim that ϕ is sometimes to *say* "It might be that ϕ," even if one lends no credence to the proposition that ϕ. Here the 'might' statement is used *purely* as a gentle way of structuring further inquiry: the teacher and student will often go on to see that the proposition that ϕ is false, perhaps by seeing what would follow from it. Thus the teacher uses the 'might' statement without intending for it to change the conversational participants' levels of credence in the proposition that ϕ, because the teacher believes that $\neg\phi$. But the context change effects of the statement still obtain, and encourage inquiry to proceed in the expected way. I think it is safe to construe this sort of use of epistemic 'might' statements as parasitic on the more standard uses we have already considered.

Presupposition and wide scope negation

As we saw earlier, some epistemic modals can be embedded under wide scope negation:

(4) John couldn't be in his office. ($\neg\Diamond\phi$) (...So he must be in the lounge.)

(32) John might not be in his office. ($\Diamond\neg\phi$)

(43) John doesn't *have* to be in his office. ($\neg\Box\phi$) (...After all, he might be in the lounge.)

(44) John needn't be in his office. ($\neg\Box\phi$) (...After all, he might be in the lounge.)

(45) John must not be in his office. ($\Box\neg\phi$)

I have already explained how to explain the credal shift induced by (4). But (4) also ensures that it is common ground that John isn't in his office. So we do not yet have a complete story about 'not' when it scopes over an epistemic modal.

To have a convenient way to describe effects on conversational context, I will say that an ordinary, non-hedged assertion ensures that the context meets the following condition:

$$\text{Presup}(\phi)$$

That is, an assertion of ϕ normally ensures that it's presupposed that ϕ. By contrast, 'It might be that ϕ' ensures that the context meets a very different condition:

$\neg\text{Presup}(\neg\phi)$

Wide scope negation over an epistemic modal simply adds a wide scope negation *to this condition.* So 'It couldn't be that ϕ' ensures that the context meets:

$\neg\neg\text{Presup}(\neg\phi)$ (i.e., $\text{Presup}(\neg\phi)$)

If we treat 'Must ϕ' as '$\neg$ might $\neg\phi$,' then 'Must ϕ' ensures that the context meets

$\neg\neg\text{Presup}(\neg\neg\phi)$ (i.e., $\text{Presup}(\phi)$)

which is the right result: as I noted earlier, 'Must ϕ' normally makes it presupposed that ϕ. And we have the right condition for sentences like

(43) John doesn't *have* to be in his office.

Namely:

$\neg\neg\neg\text{Presup}(\neg\neg\phi)$ (i.e., $\neg\text{Presup}(\phi)$)

2.3. The force and assessment of epistemically hedged statements

The probabilistic semantics that I have presented resembles traditional truth-conditional semantics in important respects. But we should not let those resemblances obscure one crucial difference: propositions represent ways the world might be, but type $\langle\langle st, v\rangle, t\rangle$ functions generally do not. For example, no difference between two ways the world could be corresponds to the difference between admitting only functions that map the proposition that map ϕ to values above $1 - \mu$ and admitting functions that map that proposition to values above μ. So the move away from truth-conditional semantics is also a move away from a semantics that is amenable to treating assertion as a kind of representation. What, then, do we do when we assert that ϕ? And what are the norms that assertions are answerable to? These questions are tightly bound up with each other. It's imprudent to neglect the *differences* between the norms that govern uses of 'Might ϕ,' 'Must ϕ,' and 'ϕ' simpliciter. But it's also imprudent to neglect our (at least prima facie) obligation to give a relatively *unified account* of that diversity.

Here is a first attempt to answer the first question:

> In asserting that ϕ, a speaker advises her addressees to conform their credences to the semantic value of 'ϕ'.

This answer fails because it overestimates the modest intentions that speakers often have when they use epistemically hedged statements. Recall the car keys case that I presented earlier: I do not know where my car keys are, and neither does my wife; she does not know where I've looked; she says "Your keys might be on the kitchen table." In many cases she will have spoken appropriately even if I have already searched the kitchen table and know that my keys are not there. I can't criticize her for giving bad doxastic advice. So my wife intends her advice to have *no force* if I already know that the keys are not on the table. She is advising only that I not *inadvertently* rule out or overlook the possibility that my keys are on the kitchen table.

If we were willing to forgo the hope that we could give a unified theory, we might start with

> In asserting that it might be that ϕ, a speaker *weakly* advises her addressees to conform their credences to the semantic value of 'Might ϕ'.

We could go on to give further clauses for other epistemic modals and adjectives. Such a theory would be tedious. More importantly, it would miss an interesting generalization that connects features of a wide range of modals. With respect to strength of advice, 'might' is like 'doesn't have to be,' 'must' is like 'couldn't be,' and so on: it's much more committal to say that the keys couldn't be in the living room than it is to say that they don't have to be in the living room, and it is still more committal to come right out and say that they *aren't* in the living room.

This leads to the following generalization.

> STRENGTH REFLECTS SPECIFICITY:
>
> The *strength* of the advice associated with an assertion of 'ϕ' reflects the *specificity* of the doxastic advice associated with 'ϕ'.

A simple assertion with no epistemic hedges is maximally specific: the content of such an assertion is the characteristic function of a set of functions from propositions to a point value—1. Any epistemically hedged statement will be less specific than one that is not hedged. But even epistemically hedged statements exhibit *degrees* of specificity: a set of functions from propositions to values in $(1-\mu, 1]$ is more specific than a set of functions from propositions to values in $[\mu, 1]$, because the length of the first interval

is greater than the length of the second. This is in line with the fact that 'Must ϕ' is more committal than 'Might ϕ,' which in turn I explain by saying that the advice associated with 'Must ϕ' is stronger than the advice associated with 'Might ϕ.' There is something intuitively attractive, I think, about connecting strength and specificity in this way: 'It might be that ϕ' admits such a wide range of credence assignments that by saying it a cooperative speaker signals that she does not have the epistemic authority to say anything that is particularly committal about whether ϕ. It also lets us explain why my wife's suggestion that the keys might be on the table is not criticizable in the ways that non-hedged assertions are. Finally, this hypothesis goes some way toward explaining why 'It must be that ϕ' is weaker than 'ϕ' simpliciter. On my view, non-hedged assertions are very special things: they are the limit case in which 'advice' becomes something like a command to *set* one's credence to a *point* value.[28] It's no wonder that the advice that I claim is associated with 'It must be that ϕ' is generally weaker than this.

2.3.1. Assessment

We pretty routinely say that epistemically hedged statements are true or false. What do these judgments amount to? On my view truth value judgments are, in general, a way of expressing a kind of approval or disapproval that may or may not latch on to properly semantic features. And I say that 'truth value' judgments about epistemically hedged statements *in fact do not* latch on to semantic features. On my view, we evaluate epistemically hedged statements in the way we evaluate other advice. Quite generally, whether a piece of advice seems all things considered appropriate to some assessor can (but needn't) depend on whether the advisor has behaved responsibly in giving the advice, on whether the assessor of the advice is in a relevantly better epistemic position than the advisor, and so on. Similarly for doxastic advice. For example, if we informally gloss the advice that is conveyed by 'It might be that ϕ' as advice not to overlook the possibility that ϕ, then whether that advice seems all things considered appropriate to some assessor can (but needn't) depend on whether the speaker behaved responsibly in advising that the addressee not overlook the possibility, on whether the assessor of the statement is in a relevantly better epistemic position than the speaker, and so on. So some uses of epistemic 'might' statements must meet relatively high standards

28. Treating this point value as 1 obviously involves some idealization; I readily acknowledge that addressees 'take on board' non-hedged assertions without becoming fully certain of their content.

to be appropriate; others are appropriate even though they meet only relatively low standards. The standards for appropriate use of "Iraq might have weapons of mass destruction" are very different from the standards for appropriate use of "It might drizzle tomorrow."

Eavesdropping cases like the one I mentioned earlier are often used to motivate a revisionary story about the *content* of epistemically hedged statements.

EAVESDROPPING:

> The White spies are spying on the Red spies, who are spying on the gun for hire. The gun for hire has left evidence suggesting that he is in Zurich, but one clever White spy knows that he is in London. After finding the planted evidence, one Red spy says to the others, "The gun for hire might be in Zurich," and the others respond "That's true." The clever White spy says "That's false—he's in London" to the other White spies, and explains how he knows this.

One argument a relativist could give here is that the truth value of 'what is said' must be sensitive to the assessor's epistemic position to explain the ways in which judgments about the Red spy's utterance are affected by the assessor's epistemic position. But if we treat a 'truth value judgment' as an expression of approval or disapproval the object of which is a *particular speech act—not* as a judgment that must be about the *content* of that speech act—then we can reject the inference from assessor relativity of judgments to assessor relativity of content. That this inference is suspect is, I think, fairly well known. But thinking of epistemically hedged statements as doxastic advice lets us not only reject the inference but also explain the relevant phenomena.

Suppose Alice gives Bert some advice. She does not know everything about Bert's situation, but she does her very best given what she does know; she takes herself to act wholly appropriately in giving that advice. Time passes. Alice learns more about Bert's situation and finds herself reflecting on the advice she had given him earlier, in her earlier state of relevant ignorance. Suppose she realizes that if she knew then what she knows now, she would have given him significantly different advice. She thinks she gave Bert bad advice. But Alice can think this without thinking that she is *criticizable* for having given him that bad advice—after all, she did her very best given what she knew at the time.

Similarly, the Red spy's doxastic advice to lend some credence to the proposition that the gun for hire is in Zurich is bad advice, and the clever White spy knows this.

But this *doesn't* mean that the Red spy is criticizable for his bad advice—assuming, of course, that he has been sufficiently diligent in acquiring and assessing pertinent evidence and that the stakes are sufficiently low. Thus I can acknowledge that the Red spy's utterance of "The gun for hire might be in Zurich" deserves a kind of disapproval, insofar as it was bad advice. At the same time it deserves a kind of approval because it was *weak* advice—the Red spy was not claiming much epistemic authority in the first place—and it was (we can suppose) the best advice the Red spy was in a position to give.

2.3.2. Seeing and overlooking possibilities

Obviously my approach makes crucial use of probabilistic tools, and it is most naturally complemented by a theory according to which belief states are modeled using probability spaces. But one part of the force of 'might' statements cannot be straightforwardly captured if we appeal *only* to transitions between probability spaces. I have in mind here the change that occurs when a believer has been *overlooking* the possibility that ϕ, and comes to *see* that possibility. Let me give an example of this kind of change.

I crack eggs with one hand, and have done so for some time. I only recently thought about *how* I crack eggs, and in thinking about it I realized that I always hold the large end of the egg in the palm of my hand, with the small end in my fingers. But once I realized this, I also realized that I believe this is the *right* way to hold an egg that you'll crack with one hand. After all, it's easier to lift the small end with your fingers than it would be to lift the large end. As ordinary speakers, we might be a little reluctant to say that, even before I thought about it, I believed that that is the right way to hold an egg that you'll crack with one hand. But those who favor probabilistic representations of belief states will say that in this case I simply assigned high credence to the proposition that ϕ—as my actual behavior regularly indicated—without realizing that I assigned high credence to that proposition.

When I thought about how I crack eggs, and realized that I always hold them in a particular way, I had a very modest "Aha!" feeling. It was modest for all sorts of reasons, of course. But one was this: *There was no change* in my level of credence in the proposition that the right way to hold an egg you'll crack with one hand is with the large end in your palm. However modest it was, I did have an "Aha!" feeling, and that feeling is a symptom of what I mean by 'coming to see a possibility one has been overlooking.' I made no conscious distinction between different ways of holding eggs, and when I realized that my behavior nevertheless does make such a distinction, I saw

a possibility I had been overlooking.

There is a significant intuitive difference between the state I was in when I overlooked the possibility that I always hold eggs in a certain way, and the state I was in when I first saw this possibility. I think that those "imperialistic apostle[s]" of Bayesianism who "insist that every sin and virtue in confirmation theory should be explained in [purely] Bayesian terms" (Earman 1992, 1) go wrong by ignoring this difference. (For example, understanding how the transition from overlooking to seeing a possibility affects credences might well shed light on the problem of old evidence.) But here I just want to note that we must be able to *model* the difference, at least, in order to describe what happens when it occurs to someone that it might be that ϕ. For as the egg cracking example shows, I can come to see a possibility without changes in my credence with respect to that possibility. And epistemic 'might' statements often cause us to see possibilities we were overlooking: "Careful, she might capture your pawn *en passant*"; "You might offend him by trying to help"; "I'm sure they haven't forgotten—they might be trying to surprise you."

How should we analyze the differences between overlooked and seen possibilities? Suppose we start with a probability function defined over a set of possible worlds. The value of the function, for a particular possible world as argument, represents the degree to which the believer believes that world is actual. If we limit ourselves to first-order credences, this approach will have nothing helpful to say about what it is to overlook and see possibilities. For if we say that to overlook a possibility is to assign it low credence, we will wrongly conflate overlooking the possibility that ϕ with believing it to be false that ϕ. And we will also wrongly rule out of court a believer's overlooking *both* the possibility that ϕ and the possibility that $\neg\phi$. If we say that to overlook a possibility is to assign it middling credence, on the other hand, we will wrongly conflate overlooking a possibility with being genuinely undecided about whether that possibility is actual.

Neither is overlooking a possibility like lacking "resiliency" or "robustness" in one's first-order credence. Resiliency and robustness are measures of the degree to which a believer's credence in a proposition is stable in the light of new evidence.[29] But whether or not a believer sees some possibility, she may have little idea what credence she ought to assign to it—and hence be in a doxastic state that is not resilient with respect to that possibility.

Finally, it will not do to model just the *transition* from overlooking to seeing a

29. See Jeffrey 1983, §12.7, Skyrms 1977 and 1980a, Lewis 1980, and the postscript to Lewis 1976a.

possibility—for example, by saying that the transition is a temporary swing toward middling credence, or that it is a temporary dip in the resiliency of one's credence. What we want is a distinction between *distinct states:* the state a believer is in when she overlooks a possibility, and the state she is in when sees that possibility.

Higher-order beliefs give the Bayesian a marginally more promising strategy. Perhaps to overlook the possibility that ϕ is to be relatively unopinionated about one's credence in the proposition that ϕ, and to see the possibility that ϕ is to be relatively opinionated about one's credence in that proposition. This proposal derives what plausibility it has from the idea that I went from assigning high credence to the proposition that the right way to crack an egg is *this* way, and low credence to the proposition that I assigned high credence to that proposition, to assigning high credence to both propositions. I thereby *realized* that I (in some sense) *thought all along* that the right way to crack an egg is with the large end in your palm. This approach would also let us distinguish between overlooking the possibility that ϕ and either believing it to be false that ϕ or being genuinely ambivalent about the possibility that ϕ. And it would let us hold that a believer can overlook both the possibility that ϕ and the possibility that $\neg\phi$, since clearly one can be relatively unopinionated about one's degrees of belief in both the proposition that ϕ and the proposition that $\neg\phi$.

But higher-order beliefs also make some problems with probabilistic representations of belief states particularly acute. It is prima facie much harder to say what would make it the case that I believe to degree 0.8 that I believe to degree 0.9 that it rained in Seattle yesterday than it is to answer the already hard questions about what fixes first-order levels of credence. And answering this question is even harder if we think, as many do, that we are in some sense idealizing when we say that believers like us have point-valued degrees of first-order belief. If it is only in an idealized sense that I believe to degree 0.9 that it rained in Seattle yesterday, then what could the *content* of my second-order beliefs about my credence in that proposition possibly be?[30] At any rate, here I simply want to leave this proposal as an open possibility—one way in which we might try to analyze the change a believer undergoes when she moves from overlooking to seeing a possibility.

For present purposes, at any rate, we do not need an analysis of the distinction between these states. Rather, we need a tractable formalism that can represent the

30. These problems notwithstanding, Bayesians clearly need *some* story about higher-order beliefs. For some work in that vein, see Mellor 1980a, Skyrms 1980b, Gaifman 1986, and Sahlin 1994.

distinction between states in which a believer overlooks and states in which she sees that possibility. That formalism should not prematurely rule out any particular analysis of this distinction, and is constrained by three further criteria:

1. It must be compatible with the (synchronic) Bayesian tools we rely on in theorizing about the effects that speech acts have on belief states and on conversational context.

2. It must provide a way to translate between credences assigned to seen possibilities and credences assigned to overlooked possibilities.

3. It must allow that we can overlook possibilities entailed by possibilities we see, and that we can see possibilities entailed by possibilities we overlook.

The formalism that I go on to provide meets these criteria, and thus begins to respond to the worry that the broadly Bayesian framework I use to describe the effects of 'might' statements unjustifiably prejudges questions about what effects such statements can have. Because I am leaving open some important questions about the proper interpretation of the formalism, one might well worry that the distinction between overlooked and seen possibilities will not yield to a Bayesian *analysis*. I grant the possibility. But we can mine insights with Bayesian tools even if on their own they do not completely characterize our cognitive lives, and even if they misrepresent us in certain sufficiently limited respects.

I want to avoid misleading uses of 'belief' and 'believes,' since the words are laden with the influence of ordinary usage. So instead I use **commitment** as a technical term for our high credence attitude, whether the credence in question is with respect to a seen possibility or an overlooked one. 'Commitment' also has misleading connotations, of course. But here is a use that may help focus intuitions: "I didn't realize it, but yes, my endorsing that theory does mean that I am committed to the claim that ϕ."

I model a single believer using two different probability spaces. One is defined over both those possibilities she overlooks and those she sees, measuring her credences with respect to all those possibilities. This space measures not only her high and low credences, but also her credences that fall short of commitment to a possibility or commitment to its complement. I call this her **fine credal space**. The other probability space is defined only over those possibilities she sees, representing (for any normal person) a proper subset of the credences represented by her fine credal space. This space again characterizes both high and low credences and middling credences, but does not char-

acterize her credences with respect to any possibilities she overlooks. Accordingly, I call this her **coarse credal space**.

Now if we think of fine credal spaces as nothing more than functions from a domain of possible worlds into $[0, 1]$—a way of thinking that is often encouraged by informal presentations of the probability calculus—then it is hard to see what coarse credal spaces could be. In fact the measure function of a probability space is a function from an *algebra* into $[0, 1]$, where $\mathcal{F}$ is an **algebra** over a set W just in case $\mathcal{F}$ is a set of subsets of W, $W \in \mathcal{F}$, and $\mathcal{F}$ is closed under complementation and union. So when we say, informally, that we have a probability space defined over a domain of possible worlds, what we really mean is that its measure function is defined over the sets in the power set of those possible worlds—in effect, over all the possible worlds propositions that have that set of possible worlds as their domain.[31] This fact lets us treat the domain of the coarse credal function as a straightforward *subset* of the domain of the fine credal function. And given a probability space $\mathbb{P}$ and any subalgebra $\mathcal{S}$ of the propositions measured by $\mathbb{P}$, we can construct another probability space, defined over exactly the propositions in $\mathcal{S}$, that agrees with $\mathbb{P}$ on the measures of those propositions. (See the appendix for a proof.) So we can construct the coarse credal space out of the fine credal space and the algebra of seen possibilities, confident that its measure function will agree with the fine credal space on the values assigned to any proposition that is measured by both spaces. Less formally: Given an algebra of seen possibilities, we can construct a coarse credal space defined over just those possibilities that accommodates a fine credal space like a map accommodates an overlay. Just as an overlay can add information without conflicting with the information represented by the underlying map, a fine credal space adds information about behavioral dispositions without conflicting with the 'seen credences' represented by the coarse space. We can now straightforwardly represent the distinction between seeing and overlooking a possibility. If a believer comes to see the possibility represented by u, we include u as

31. In a number of places, including his 1981, 1986 and 2005, Stalnaker suggests that we represent a doxastic state by partitioning a relatively unrestricted domain of possible worlds into equivalence classes, or 'cells,' such that 'cell-mates' are worlds that the believer in question does not distinguish between. (See also Edgington 1995, 266, and compare Kripke 1980, 15–20.) My proposal here is formally similar to Stalnaker's, but we aim to describe different phenomena. For example, the egg cracking case shows that it's possible for a believer's behavioral dispositions to distinguish between possibilities that she overlooks. Although I endorse Stalnaker's approach, and it is compatible with my proposals, for ease of exposition I will pretend that the atoms of the fine credal space are the singletons of possible worlds.

an atom in the algebra of her coarse probability space. Similarly, if a believer comes to overlook (or ignore) a possibility, we coarsen her coarse credal space, from a probability space defined over an algebra $\mathcal{F}$ to one defined over an appropriate subalgebra of $\mathcal{F}$. The intuitive justification for this treatment is that the coarse credal space should not take a stand on overlooked possibilities. So it leaves those possibilities unmeasured.

The space of seen possibilities is an algebra, so the set of propositions over which a given coarse credal space is defined is closed under complementation and union. And hence any such set is closed under Boolean operations generally. This has a number of consequences that are relevant to our purposes here. To begin with, my framework cannot represent a believer who sees the possibility that ϕ and sees the possibility that ψ but overlooks a possibility yielded by any Boolean operation on the proposition that ϕ and the proposition that ψ. For example, I cannot represent such a believer if she overlooks the possibility that $\neg\phi$, or overlooks the possibility that $\phi \vee \psi$, or overlooks the possibility that $\phi \wedge \psi$, or overlooks the possibility that $\phi \vee \neg\phi$, or .... For many cases I do not think that this limitation of the framework is implausible or unwelcome. In virtue of seeing the possibility that the right way to crack an egg is with the large end in your palm, I see the possibility that the right way to crack an egg is not with the large end in your palm. In virtue of seeing the possibility that the right way to crack an egg is with the large end in your palm, and seeing the possibility that the right way to crack an egg is with the small end in your palm, I see the possibility that the right way to crack an egg is with the large end in your palm or with the small end in your palm. Think of the closure properties in this way: Because each seen possibility *partitions* logical space, each seen possibility *lays boundaries* on logical space. The framework has it that any proposition whose boundaries can be defined purely in terms of the boundaries laid down by seen possibilities is itself a seen possibility.

Of course there are cases for which it's not obvious whether this kind of closure property is unproblematic. For example, according to the framework all believers see any proposition that is true in all the worlds in W, and assign any such proposition full credence. So the treatment I offer here does not on its own explain how to handle 'might' statements like

(46) It might be that every even number greater than two is the sum of two primes.

But the framework can model believers who overlook *non-Boolean* entailments of possibilities they see, and it can model believers who overlook non-Boolean entailers of

possibilities they see. For example, suppose that t entails u, and u entails v. Suppose also that our believer sees the possibilities represented by s, t, and v, but overlooks the possibility represented by u. Then the coarse credal space will measure any subset of W that partitions W solely along solid lines in FIGURE 1, but will omit those subsets that partition along any dashed line. Notice that the coarse space thus leaves unseen

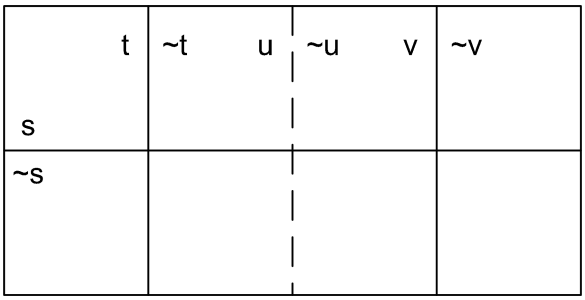


FIGURE 1

the appropriate non-Boolean entailers and entailments of seen possibilities. This is important because I may see the possibility that my partner castles, for example, while overlooking the possibility that my partner castles or moves *en passant*. Moreover, the framework might go some way toward reconciling the folk conception of belief with the fact that, according to probabilistic models of belief states, our beliefs are closed under entailment. For example, we might say that our fine-grained commitments are closed under entailment, although often we do not *see* all those commitments. I find much about this line attractive, but I will leave its development for another time.

Beyond the closure properties already discussed, the framework puts no unusual constraints on the norms, if any, that govern the relationships between overall doxastic states and the possibilities a believer sees and overlooks. In light of this neutrality it is important to be clear about what work the framework does. When it occurs to someone that it might be that ϕ, often she will not be sure whether or not ϕ—which suggests that we will need a probability space to model her credences—*and* she will come to see the possibility that ϕ. To describe all the effects of epistemic 'might' statements, we need a way to represent both of these changes, and we need to allow that the changes can occur independently of each other. Earlier I raised the worry that we cannot represent the distinction between overlooked and seen possibilities with a probability space. The framework gives us a modest way to defuse this worry, for the time being: we represent a belief state using *two* probability spaces that agree on all the credences measured

by both. We can tackle the *analysis* of the distinction between overlooked and seen possibilities another time.

2.4. Conclusion

The analyses that I have proposed here are incompatible with two common assumptions that guide work on the semantics and pragmatics of natural language. The first is that the effects that utterances have on doxastic states and on conversational context are fundamentally quite similar. The second is that these effects are not degreed, and thus can be accurately described in binary terms: worlds are either 'ruled in' or 'ruled out' of belief and context sets. It is hard to overstate how fundamental these assumptions are. The second assumption in particular has contributed significantly to the tractability of formal semantics, because it has let us do a wide range of interesting work without incurring the complications involved with a degreed type theory. A purely truth-conditional treatment of quantification, to take one example, is much simpler than the treatment I have offered here. And for practical reasons I think there's much to be said for working with simpler treatments where possible. So in this respect I am not rejecting truth-conditional theorizing altogether, despite rejecting assumptions that underlie it.

Moreover, I hope it is clear that my approach can absorb much of truth-conditional semantics as a special case. But this 'theoretical absorption' has ramifications elsewhere. Because the content that I associate with epistemically hedged sentences is not representational, we need a new way to think of the force with which that content is put forward. I argued that asserting an epistemically hedged sentence is a way of giving doxastic advice, and discussed some phenomena that that hypothesis would help explain. Insofar as we would like a theory that unifies the speech act associated with epistemically hedged sentences and the speech act associated with non-hedged sentences, we are compelled to say that to assert a non-hedged sentence is to give doxastic advice, too: '*Believe* that ϕ,' as opposed to 'Lend at least credence μ to ϕ.' This way of thinking about assertion of non-hedged sentences was always in principle available, I suppose, but without considering epistemically hedged sentences it would have been hard to see any reason to prefer it to thinking of assertion as a kind of representation.

In the 1970s, semantic proposals were generally given for a *fragment* of a natural language. The tacit codification of the methods of formal semantics has since encouraged many to forget that fruitful semantic techniques and frameworks are *fruitful rel-*

ative to such a fragment. Indeed, the worth of a framework for a particular fragment may be downright misleading when we begin to consider other and larger fragments of a language. I suspect that this is the case with the language of subjective uncertainty.

2.5. Appendix: Constructing coarse credal spaces

Probability spaces are triples $\langle W, \mathcal{F}, \mu \rangle$ such that:

1. $\mathcal{F}$ is an algebra over W;
2. μ is a function from $\mathcal{F} \to [0, 1]$;
3. $\mu(W) = 1$;
4. If M and N are disjoint elements of $\mathcal{F}$, then $\mu(M \cup N) = \mu(M) + \mu(N)$.

Given a probability space $\langle W, \mathcal{F}_f, \mu_f \rangle$ and a subalgebra of $\mathcal{F}_f$, $\mathcal{F}_c$, we can construct another probability space $\langle W, \mathcal{F}_c, \mu_c \rangle$ that agrees with $\langle W, \mathcal{F}_f, \mu_f \rangle$ on the measures of all the sets in $\mathcal{F}_c$.

> Proof. By assumption $\langle W, \mathcal{F}_f, \mu_f \rangle$ is a probability space. Let $\mathcal{F}_c$ be an arbitrary subalgebra of $\mathcal{F}_f$ over W. Trivially $\mathcal{F}_c$ is an algebra over W, and because $\mathcal{F}_c$ is a subalgebra of $\mathcal{F}_f$, $\mathcal{F}_c \subseteq \mathcal{F}_f$. Construe the function μ_f as a set of ordered pairs, where the first member of each ordered pair is a set in $\mathcal{F}_f$, and the second member is in the interval $[0, 1]$. Construct μ_c to include exactly the ordered pairs in μ_f whose first members are elements of $\mathcal{F}_c$. Then μ_c agrees with μ_f on the values assigned to sets that are in $\mathcal{F}_c$. And $\langle W, \mathcal{F}_c, \mu_c \rangle$ is a probability space, because
>
> 1. $\mathcal{F}_c$ is an algebra over W;
> 2. μ_c is a function from $\mathcal{F}_c \to [0, 1]$ (because μ_f is a function into $[0, 1]$, and $\mu_c \subseteq \mu_f$);
> 3. $\mu_c(W) = 1$ (by the construction of μ_c, given that $W \in \mathcal{F}_f$ and $\mu_f(W) = 1$);
> 4. If M and N are disjoint elements of $\mathcal{F}_c$, then $\mu_c(M \cup N) = \mu_c(M) + \mu_c(N)$ (by the construction of μ_c, given that $\mu_f(M \cup N) = \mu_f(M) + \mu_f(N)$). ⊣

CHAPTER 3

Lessons from the Context Sensitivity of Causal Talk

Suppose we have a theory of singular causation according to which

(1) Caesar's birth was a cause of his death.

is true.[1] Charge: It offends common sense to say that Caesar's birth was a cause of his death. Response: The assertibility conditions of causal claims are affected by conversational context. Even if (1) is true, in normal contexts it will be uninformative, or misleading, or not a suitable answer to the sorts of questions we are interested in. And general pragmatic principles explain why it would offend common sense to assert even true sentences that are uninformative, misleading or not topical. So it is no mark against a theory of causation that it predicts that (1) and certain other odd sounding sentences are true (Lewis 1973, 162, Bennett 1995, 130–133, and Lewis 2000, 100–101).

This response is based on the plausible idea that some distinctions made in natural language need not—indeed, *should* not—be reflected in metaphysics. Natural lan-

1. Mackie 1980, Lewis 1973 and 2000, and Bennett 1988 offer such theories. I discuss only singular causation in this chapter. To discourage general causation readings of examples I often use the simple past tense, as I do here.

guage does distinguish between Caesar's birth and Brutus's stabbing, with respect to being a cause of Caesar's death, but perhaps our metaphysics of causation should not. If we pursue this line, as I think we should, then we must ask which natural language distinctions do constrain our metaphysics, and how. These questions are especially important for distinctions that are sensitive to features of conversational context, because we should not inadvertently impute the effects of such context sensitivity to our metaphysics.

This chapter starts by arguing that ordinary causal talk is far more sensitive to conversational context than has been recognized to date. I then formulate a principle that helps characterize that context sensitivity. I argue that this principle explains at least some of the oddness of 'systematic causal overdetermination,' and also that it explains why some putative overgenerated causes are never felicitously *counted,* in conversation, as causes. These explanations are a natural extension of the line that Lewis, Bennett, and others take with "Caesar's birth was a cause of his death": when we are confronted with linguistic data that threaten to make trouble for our metaphysics, we try to give a plausible explanation of the data that does not require any changes to our metaphysics. And when we are successful, it's permissible *not* to change the metaphysics. The explanations that I offer here, however, make metaphysical theorizing about causation much less constrained by ordinary language judgments than we might have thought. As a result, though causal talk and the metaphysics of causation are both of independent interest, they are not well investigated independently.

3.1. The context sensitivity of causal talk

Philosophers routinely observe that what count as *the* causes of an event, in a conversation, is a dramatically context sensitive matter.[2] In light of this they suggest that our intuitions about causation should not be influenced by judgments about sentences of the form 'c was among the causes of e.' We should instead restrict our attention to putatively less context sensitive sentences of the form 'c was *a* cause of e.' This suggestion is underwritten by a tacit argument by analogy. Which books count as *the* books, in a conversation, is a dramatically context sensitive matter. But even if what counts as *a* book is a little context sensitive, 'a book' is much less context sensitive than 'the books.' By analogy, 'a cause of e' is much less context sensitive than 'the causes of e.'

2. I assume throughout that the causal relata are events, though I do not always use causal attributions that make this assumption manifest.

Our work on the metaphysics of causation should be guided by the least context sensitive expressions we can find, so we should focus on 'c was a cause of e.'[3] This argument is seductive. But it is specious, at least because the analogy fails.

To begin with, a given definite description of the form 'the Fs of DEFINITE NP' is generally much more specific than its 'the Fs' counterpart: 'the friends of Dave,' for example, is much more specific than 'the friends.' This specificity means that definite descriptions whose nominals have genitive modifiers are generally *much less context sensitive* than their counterparts without such modifiers—unless there is another reason for their context sensitivity.[4] (Continuing the example, 'the friends of Dave' is much less context sensitive than 'the friends.') In light of this it isn't obvious that 'the causes of e' is as dramatically context sensitive as it is *simply* because it is a definite description. And if its context sensitivity has another source, then it would not be surprising to find that 'a cause of e' is context sensitive, too.

In fact this is what we do find. For an event to count as a cause of e in a context is for it to count as *among the causes* of e in that context. So 'a cause of e' is *every bit* as context sensitive as 'the causes of e,' and in just the same ways. This is because to count as an F of a in a context is, quite generally, to count as among the Fs of a in that context. Consider

DIALOGUE 1

Max: Al, Betty, and Clara are the deans of State U.

Nancy: Doug is a dean of State U., too.

Here Nancy has disagreed, to some extent, with Max. To be a dean of State U. is to be one of the deans of State U. If Al, Betty, and Clara are the deans of State U., then Doug is not a dean of State U. But Nancy believes that Doug is a dean of State U. So, although Nancy does not deny that Al, Betty, and Clara are each a dean of State U., she does insist (contra Max) that the deans of State U. are Al, Betty, Clara, and Doug. And she conveys this by saying that Doug is *a* dean of State U. By contrast, consider

3. To his credit, Lewis admits that "even 'a cause of' may carry some hint of selectivity" (1986a, 216). In his 1977 and 1984, Unger argues that the verb 'cause' and "other transitive causal verbs" are context sensitive, but does not discuss 'a cause of e.'

4. It is interesting that most of Russell's examples of definite descriptions have nominals with genitive modifiers. He singles them out as "descriptive functions," including "the father of x," "the sine of x," "the present King of France," "the author of *Waverly*," "the centre of mass of the Solar System at the first instant of the twentieth century" and so on (1905, 35; 1919, 323). Definite descriptions of this form seem to encourage 'attributive' over 'referential' readings, in something like Donnellan's senses (1966).

DIALOGUE 2

Max: John bought the books.
Nancy: Karen bought a book, too.

Though Nancy here aims to convey new information to Max, she needn't convey any disagreement with him. If 'an F of a' behaved just like 'an F,' and 'the Fs of a' behaved just like 'the Fs,' then this feature of DIALOGUE 2 would strongly suggest that in DIALOGUE 1 Nancy did not express any disagreement with Max. But she did. We can easily generate countless more examples like these. So, in general, to count as an F of a in a context is to count as one of the Fs of a in that context.[5]

'The causes of e' and 'a cause of e' are no exception to this generalization. Notice that Nancy can express disagreement with Max by using a causal indefinite description: 'c was a cause of e.'

DIALOGUE 3

Max: The ice and the brakes' failure were the causes of the accident.
Nancy: The driver's fatigue was a cause of the accident, too.

Max attributes full causal responsibility for the accident to the ice and the brakes' failure, whereas Nancy thinks that the driver's fatigue was a cause too. By counting the driver's fatigue as *a* cause of the accident, Nancy conveys that she considers the fatigue to be *one of the* causes of the accident. She uses 'a cause' because she wants to *concede* that the ice and the brakes' failure are partially responsible for the accident, while adding that the driver's fatigue played a significant enough role that it should count as one of the causes, too. So to count as a cause of e, in a context, is to count as one of the causes of e in that context. And this means, again, that 'a cause of e' is exactly as context sensitive as 'the causes of e.' If we want to insulate our intuitions about causation from the context sensitivity of 'the causes of e,' then focusing on 'a cause of e' will not help at all.

5. Examples like (2) and (3) are not counterexamples to this generalization, because the definite descriptions are singular.

(2) Put your cup down on the arm of your chair.

(3) He married the daughter of his bank manager.

For discussion of (2), (3), and similar sentences, see HUDDLESTON & PULLUM 2002, 369 and GRAFF 2001, 37.

Indeed, our theorizing about causation has been *led astray* by neglect of the ways in which 'a cause of e' is context sensitive. To bring this out I want to look at some linguistic data that clearly should not be accounted for in our metaphysics. For this reason these data put nonnegotiable demands on the linguistic theory that interfaces between our metaphysics and our ordinary language judgments: the linguistic theory *has* to account for these data on its own. But as we will see, it's plausible that a theory powerful enough to do this work can also do work usually taken to be the metaphysician's responsibility.

First let me be clear about the kind of context sensitivity that matters for present purposes. I am interested in how the assertibility conditions of sentences of the form 'c was a cause of e' are sensitive to conversational context. By the **assertibility conditions** of a sentence I mean the conditions in which—that is, the circumstances and the conversational contexts in which—it is appropriate for a speaker who knows all the relevant non-semantic facts to use that sentence. There is no doubt that the assertibility conditions of causal claims are sensitive to conversational context, in the familiar way that the assertibility conditions of any sentence are sensitive to conversational context: clearly it's often inappropriate to say rude things, or things that have already been said, or things that are manifestly obvious, or .... What I want to call attention to here is *one unnoticed way* in which the assertibility conditions of 'c was a cause of e' depend on conversational context.

Suppose I know that the leak caused the puddle, and that the puddle together with the cold caused the ice (FIGURE 1). I tell you about the ice, and you ask about its causes.

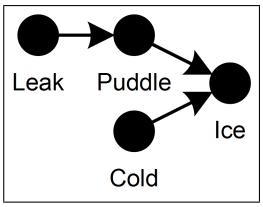


FIGURE 1

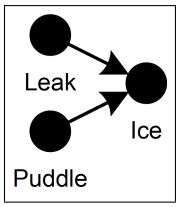


FIGURE 2

I could begin to answer your question with any of:

(4) The leak was a cause of the ice. (...So if we fixed the leak...)

(5) The puddle was a cause of the ice. (...So if we fixed the drain...)

(6) The cold was a cause of the ice. (...So if we fixed the heater...)

(7) The leak was a cause of the ice, and the cold was a cause of the ice.[6]

But I could not appropriately say

(8) #The leak was a cause of the ice. The puddle was also a cause of the ice.

(9) #The puddle was a cause of the ice. The leak was also a cause of the ice.

Assertions of (8) and (9) would not appropriately describe FIGURE 1's causal structure. Rather, they would convey that the leak did not cause the puddle, so that we have joint causation as in (for example) FIGURE 2.

What's wrong with using (8) and (9) to describe FIGURE 1? Very, *very* roughly, they wrongly count one causal route to the ice twice. It will be easier to give a more precise and more general explanation if I introduce two bits of terminology. First, many philosophers of causation, including Lewis and Bennett, expressly attempt to characterize a "broad and non-discriminatory" causal relation (LEWIS 1973, 162). For example, such accounts say that throwing a switch so that a train continues down the right-hand track (instead of the left-hand track) bears this relation to the train's arrival, even if the tracks rejoin before the arrival and the switching makes no difference to the time and manner of the arrival. There is significant disagreement about the nature of this relation, but I will assume that there is a metaphysical natural kind that Lewis, Bennett, and similar philosophers of causation are aiming to characterize. For convenience I will call this putative natural kind **causal relevance**. I leave open the question which analysis of causal relevance is right, and I also leave open the question which events that are causally relevant to e can count as a cause of e in a given context. Note that causal relevance is a directed relation, because the switch is causally relevant to the arrival, but not vice versa.

Second, a sequence of events $\langle \ldots, e_{n-2}, e_{n-1}, e_n \rangle$ constitutes a **causal path** to e_n just in case e_{n-1} is causally relevant to e_n, e_{n-2} is causally relevant to e_{n-1}, and so on.[7] I want to emphasize that the "broad and non-discriminatory" nature of causal relevance means that there will be very many causal paths to any effect, and that very

6. I do not intend for my lists of appropriate and inappropriate sentences to be exhaustive.

7. The values of the subscripts here can be negative, because although every causal path *ends* in an effect, we should leave open the possibility that some causal paths do not have beginnings. In principle we should also leave open the possibility that even causal paths with beginnings and ends are more densely packed than sequences of integers, but for present purposes we can ignore that complication.

many of those paths will overlap. It is because of this overlapping that I use the term 'causal path' instead of the more familiar 'causal chain': on my definition distinct causal paths to e_n can overlap in the sense that they have events in common other than e_n. The 'chain' metaphor obscures this possibility.[8]

The principle that I will use to explain why (8) and (9) are not apt descriptions of FIGURE 1 is quite simple:

USE GOOD REPRESENTATIVES

> When you ascribe some causal responsibility for e to a causal path to e, use good representatives of that path for the purposes at hand.[9]

Clearly this principle is also vague and in some respects noncommittal, but this is appropriate to our current understanding of how causal talk works. Here I want to reduce the vagueness just enough to make the principle do some interesting work for metaphysics. As the fact that both (4) and (5) are felicitous in some contexts shows, the leak and the puddle are, in some contexts, both good representatives of the causal paths through them to the ice. The problem with (8) and (9) is that these sentences try to represent those paths using *both* the leak *and* the puddle, when either event would have sufficed. Consider just (8). Once we use the leak to represent the causal paths through the leak to the ice, the puddle is *no longer* a good representative of those paths for the purposes at hand. That is, by using the leak to represent those paths we make it the case that the puddle is not a good representative of them, because we *make otiose* whatever representative role the puddle could have played. So USE GOOD REPRESENTATIVES also explains why it is easy to hear (8) and (9) as describing the joint causation of FIGURE 2: in interpreting either sentence we assume that the speaker believes that neither the puddle nor the leak is otiose. That is, we interpret the speaker as thinking that the puddle is needed to represent causal paths that are not well represented by the leak. In particular, we infer that the puddle represents *paths that do not even include* the leak.[10]

8. For a similar use of 'path,' see LEWIS 1976b, 63–64.

9. A serious discussion of absences and omissions would take us very far afield, but I do want to note that it is easy to generate analogues of the leak/puddle case for absences. (This is a 'new' kind of context sensitivity, not obviously related to the familiar broadly normative context sensitivity we see in causal talk about absences.) USE GOOD REPRESENTATIVES might explain these cases as well: for example, 'absence' expressions might represent nonactual causal paths that, if they were actual, would consist solely of events.

10. It's worth noting that current work on causal models is orthogonal to this kind of conver-

I realize that the idea that we assign causal responsibility to causal *paths* may be surprising, and so I want to give some intuitive motivation for it. In saying 'c was the cause of e' or 'c was causally responsible for e' I do not think we generally mean that c was *in itself* causally responsible for bringing about e. After all, we know that the other events on the causal paths through c to e are partly to blame or to credit for bringing about e, in the sense that we might well have prevented or altered e by preventing or altering events *other* than c that were on the paths represented by c. For reasons that have much to do with our interests and those of the other conversational participants, we pick out c as a particularly apt representative of those causal paths. For example, c might be a morally significant part of its causal paths, or a part that it's feasible to repair, or a part such that a little change in it, holding certain aspects of the situation fixed, would have made a big change in e. But this shouldn't obscure the fact that c brings about e only with the help of other events on the causal paths through c to e, and so in a sense represents those events, too. Moreover, it is important to remember that we arrived at USE GOOD REPRESENTATIVES *via* our intuitive judgments about the leak/puddle case—and we were guided by the suspicion that there was something wrong with counting one causal route to the ice twice.

Let's consider another case. An appropriately informed speaker may appropriately describe FIGURE 3 using any of (10)–(13).

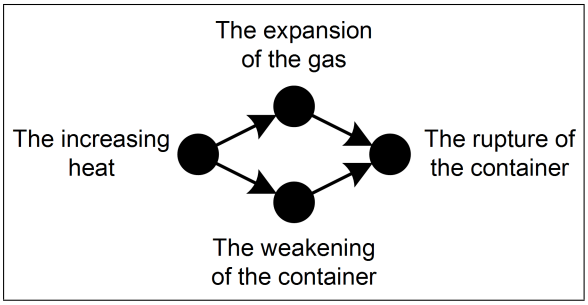


FIGURE 3

(10) The expansion of the gas was a cause of the rupture of the container.

(11) The weakening of the container was a cause of its rupture.

sational context sensitivity. (See for example the proposed semantics for 'is a cause of' in PEARL 2000, 222–223.) But the fine distinctions afforded by causal models may help us theorize about ordinary causal talk.

(12) The increasing heat was a cause of the rupture of the container.

(13) The expansion of the gas was a cause of the rupture of the container. The weakening of the container was also a cause of the rupture of the container.

But neither (14) nor (15) appropriately describes the case.

(14) #The expansion of the gas was a cause of the rupture of the container. The increasing heat was also a cause of the rupture of the container.

(15) #The weakening of the container was a cause of its rupture. The increasing heat was also a cause of the rupture.

For some purposes the increasing heat is a good representative of the causal paths through it to the rupture of the container. For other purposes, the expansion of the gas and the weakening of the container are good representatives of those paths. But (I submit) there are few if any purposes for which a speaker who knows the causal structure at issue would use the increasing heat to represent *just* the paths through the weakening of the container, leaving a need for the expansion of the gas to represent the paths through *it* to the rupture.

It's of course true that we can use sentences like (16)–(18) to describe Figure 1:

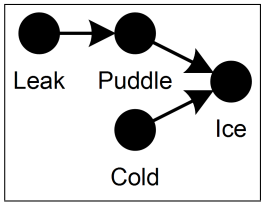


Figure 1

(16) The leak was a cause of the ice, because it was a cause of the puddle.

(17) The leak was a cause of the puddle, and was thereby a cause of the ice.

(18) The leak was a cause of the puddle, and the puddle was a cause of the ice, so the leak was a cause of the ice.

(We might use these sentences to explain why, given that we can fix neither the drain nor the heater, we ought to fix the roof.) These sentences explicitly allot two representatives—the leak *and* the puddle—to the causal paths through the leak through the pud-

dle to the ice. *Why* would we allot two representatives to these paths? Consider some contexts in which we would actually *use* (16)–(18). Typically, we use such sentences to *explain why* the leak was a cause of the ice, in speaking with someone who grants that the puddle was a cause of the ice but does not believe that the leak was, too. The causal paths that we are trying to inform our addressee about *need* (for these purposes) to be represented both by the leak and the puddle, for—as far as the addressee knows—the paths through the puddle to the ice do not include the leak. We inform addressees that some of those paths do include the leak by being explicit about the fact that we using both the leak *and* the puddle to represent those paths.

It may seem strange that there are contexts in which (4) and (5) are both appropriate, though (8) is not.

(4) The leak was a cause of the ice.

(5) The puddle was a cause of the ice.

(8) #The leak was a cause of the ice. The puddle was also a cause of the ice.

After all, (8) is just (4) followed by (5). So I want to emphasize that this sort of phenomenon is fairly routine. Suppose that yesterday in the park I saw two dogs—one the largest I've seen in months, and the other the smallest I've seen in months. I say

(19) I saw a dog in the park yesterday.

I could then felicitously say either (20) or (21).

(20) The dog was the largest I've seen in months.

(21) The dog was the smallest I've seen in months.

But (unless I haven't seen any other dogs in months) I could not felicitously say

(22) #The dog was the largest I've seen in months. The dog was also the smallest I've seen in months.

The utterance of the first sentence of (22) changes the context in a way that makes the utterance of the second sentence inappropriate, because it makes the large dog uniquely salient and thereby affects the interpretation of 'the dog' in the second sentence. The utterance of the first sentence of (8) also changes the conversational context, making the puddle an otiose (and thus poor) representative of the paths through it to the ice.

Neither case, I think, is particularly exotic.

I readily grant that the notion of "good" causal representatives does not, at this point, have the content needed to make many substantive predictions about not yet considered phenomena. But here, at least, I am not interested in making such predictions. All I want to do with USE GOOD REPRESENTATIVES is impose a little structure on the post-hoc explanations we offer for the ways in which the assertibility conditions of causal claims are influenced by conversational context. Why does that structure matter? Consider two questions:

1. Which events that are causally relevant to e can felicitously count as a cause of e, in which contexts?

2. Given a particular causal path to e, which events can felicitously represent that path, in which contexts?

Both these questions are very difficult, and both have (to say the least) a significant empirical component. We are nowhere near to having systematic answers to either of them. But the second question is *less* difficult than the first. Although it is hard (if not impossible) to compare how well *any* two events that are causally relevant to e do at counting as causes of e in a context, it is *less* hard to compare how well two events that *share* a causal path to e do at representing that path, in a context.[11] As we will see, in certain important cases on which much ink has been spilt, it is *easy* to answer this question.

3.2. Applications

3.2.1. 'Causal exclusion'

Jaegwon Kim makes a claim that I will call

EXCLUSION

> "It is at best extremely odd to think that each and every bit of action we perform is overdetermined in virtue of having two distinct sufficient causes" (KIM 1989, 247; cf. MALCOLM 1968, 52–53).

11. In part this is because we are factoring out and leaving for another time questions about *which causal paths* are felicitously represented in which contexts.

We will have different reactions to EXCLUSION depending on what we think the oddness it describes amounts to. For example, we might think that the oddness imposes certain constraints on our theory of mind—say, that we must *reject* any theory that commits us to the systematic overdetermination of our actions. Or, we might think that the oddness can be explained in independent terms that do not constrain our metaphysics of mind. The first line threatens to show too much, especially if we generalize it beyond the mental. As Brian Jonathan Garrett writes, "When the various sciences give causal explanations of the same effects, the appearance of overdetermination we are left with should be respected. These appearances require saving, not elimination or re-interpretation" (1998, 368). That is, the appearance of overdetermination and any discomfit that accompanies it require *explaining*, in a metaphysically neutral way.

Here is one way in which the oddness that worries Kim manifests itself. In the metaphysics classroom it seems that (23) and (24) are both appropriately assertible, though (25) is quite odd.

(23) The movement of the baseball's atoms was a cause of the window's shattering.

(24) The movement of the baseball was a cause of the window's shattering.

(25) #The movement of the baseball's atoms was a cause of the window's shattering. The movement of the baseball was also a cause of the window's shattering.

In light of this one might wonder: Why is (25) so odd if the sentences that make it up are in themselves both appropriately assertible? Perhaps the oddness of (25) reveals that things are not all well with one of its parts. In particular, perhaps it reveals that strictly and literally speaking the movement of the atoms was a cause of the window's shattering, but the movement of 'the baseball' was not.[12]

Recall

(4) The leak was a cause of the ice.

(5) The puddle was a cause of the ice.

(8) #The leak was a cause of the ice. The puddle was also a cause of the ice.

12. For a position that is like this in some respects, see MERRICKS 2001.

As we saw earlier, because the assertibility conditions of 'c was a cause of e' are sensitive to conversational context, there are contexts in which (4) is appropriately assertible, and (5) is appropriately assertible, but (8) is not. This shows that a pair of causal claims taken together may not be appropriately assertible in a given conversation, even if both claims taken singly are appropriately assertible in that conversation. Just as no one would think that the inappropriateness of (8) shows that strictly and literally speaking at least one of (4) and (5) is not true, we should not assume without further argument that the inappropriateness of (25) reflects badly on (23) or (24).

We still want to know *why* (25) is so odd, however, and I think that USE GOOD REPRESENTATIVES helps us get a better handle on that oddness. As we saw earlier, an assertion of (8) suggests that the leak and the puddle represent different causal paths to the ice, because the mention of both the leak and the puddle suggests that the speaker thinks they represent distinct causal paths. Similarly, a speaker who asserts (25) thereby suggests that the movement of the atoms and the movement of the baseball represent different causal paths to the ice. Because the movement of the atoms and the movement of the baseball spatiotemporally overlap, it's controversial what it means to say that we have 'different' causal paths in this sort of case. And this controversy is one reason why asserting (25) is odd. A speaker who asserts (25) on its own suggests that the movement of the atoms and the movement of the baseball represent different causal paths, but does nothing to explain her stand on that controversy, or even to acknowledge that there is any controversy that warrants consideration. (25) almost makes it sound as though the speaker thinks there's no controversy to worry about: she assumes that her addressees will know just what she has in mind when she suggests that the movement of the atoms and the movement of the baseball represent causal paths that are (in some inevitably controversial sense) 'different.'

Moreover, even if the causal paths that are well represented by the movement of the atoms and the causal paths that are well represented by the movement of the baseball are not identical as a matter of *necessity,* I think it's plausible that they are *contingently* identical in a suitably hygienic sense.[13] But I doubt that (25) has the philosophical subtlety to convey that the movement of the atoms and the movement of the baseball represent different causal paths *while leaving open* the possibility that those paths are contingently identical. So (25) *also* suggests that the causal paths represented by the movement of the atoms are not even contingently identical to the causal paths represented by the movement of the baseball.

13. For one such sense, see YABLO 1987.

There are also contexts in which both (26) and (27) are appropriately assertible, though (28) would sound odd:

(26) Mental event m was a cause of physical event p.

(27) Neural event n was a cause of physical event p.

(28) Mental event m was a cause of physical event p. Neural event n was also a cause of physical event p.

Again, we cannot read back from problems with (28) to problems with the sentences that make it up. And again, (28) conveys that m and n represent different causal paths to p, with all the controversy that involves. In particular, insofar as (28) suggests that the causal paths represented by m and n are not even contingently identical, a speaker who asserts it thereby rejects theories on which m is a determinable of n, or m superdupervenes on n, or m is realized by n, or m's causal powers are "wholly constituted by" those of n.[14] The core of the problem here is that even if $m \neq n$, a physicalist still wants m and n to be intimately enough related that they will not be on distinct causal paths to p. And a dualist may well think that (28) is not so odd.

I want to be clear about what all this does *not* show. It obviously does not show that there is no problem of mental causation. We still need to be able to explain why (26) is true. Neither does it *show* that EXCLUSION has no metaphysical upshots. But when we are faced with "extreme oddness" that *might* require invasive metaphysical surgery, the prudent thing to do is see whether we can address that oddness without having to put the patient under. That is what I have tried to do here.

3.2.2. Transitivity and overgeneration

A common move in the literature on causation, events, and causal transitivity is to argue that a theory overgenerates causes: it offends common sense by counting some event (or fact, or whatever,) as a cause of another, when, intuitively, it is not. And so the theory is deemed false, or at least 'costly.'

There is a sense in which we ought to worry about this objection only if our aim is to say what it is for an event to count as *a* cause of another in a particular conversational context. If our aim is simply to say what it is for an event to count as *causally relevant* to another, then the fact that some events that are causally relevant to e in no context

14. See, respectively, YABLO 1992, WILSON 1999, SHOEMAKER 2001, and PEREBOOM 2002.

count as a cause of e may be utterly unsurprising. Consider Lewis's suggestion that his analysis of causation is really an analysis of "causal histories," parts of which

> . . . will not be at all salient in any likely context . . . : the availability of petrol, the birth of the driver's paternal grandmother, the building of the fatal road, the position and velocity of the car a split second before the impact (1986a, 215–216).

By putting transitivity to work, we can expand Lewis's list in any number of directions. One part of the causal history of the building of the fatal road, for example, is the crew's painting of the north curb line. So the crew's painting of the north curb line is part of the causal history of the accident. Granted, in no context does it count as *a cause* of the accident. But (the arch proto-contextualist might say) so what?[15] We as metaphysicians are interested in the *metaphysics* of the *causal relevance* relation—a metaphysics that would yield sufficient conditions for the assertibility of sentences of the form 'c was a cause of e' only if it were supplemented with an appropriate (perhaps even final and complete) semantics and pragmatics.

Whether one thinks this gambit is principled will depend, I think, on what one thinks of expressions like 'causal relevance' and Lewis's "causal history." In my experience philosophers in the business of analyzing a "broad and non-discriminatory" causal relation (Lewis 1973, 162) tend to welcome the move to 'c was causally relevant to e' once they appreciate how context sensitive the assertibility conditions of 'c was a cause of e' really are. But some other philosophers doubt that there is any helpful pretheoretic content to 'causally relevant.' We can go some way toward appeasing those who think there is little to be gained by theorizing about causal relevance by giving *specific* explanations of *why* a particular event will count in no context as a cause of e, even if it is causally relevant to e.

Consider

SWITCH

> A train departs Mountain Station. It comes to a fork in the track, where an engineer flips a switch so that the train continues on the right-hand track. The right-hand and left-hand tracks rejoin before reaching Valley Station. The train arrives at Valley Station, and the switching made no difference to the time and manner of its arrival.

Ned Hall asks: "Is [the engineer's] flipping the switch a cause of the train's arrival? Yes, it is, though the opposing reaction surely tempts" (2000, 187–188). Hall gives a number

15. For similar maneuvers, see Lewis 1979 and 1996.

of reasons *to* think the flip of the switch is a cause of the train's arrival—reasons that perhaps should be construed as showing that the switch was causally relevant to the arrival, or, following Lewis, part of the causal history of the arrival. But we also want to know *why* the opposing reaction is so tempting. And we now have the means to answer just this question.

Recall

USE GOOD REPRESENTATIVES

> When you ascribe some causal responsibility for e to a causal path to e, use good representatives of that path for the purposes at hand.

And consider the sentence

(29) #The engineer's flipping the switch was a cause of train's arrival.

When a speaker uses (29), by USE GOOD REPRESENTATIVES she conveys that the flipping of the switch is a good representative of the causal paths running through it to the train's arrival. But the switch is undeniably a poor representative of those paths *compared to* many other events on those paths. For example, the departure, among other events, is for many purposes a significantly better representative of the relevant paths. So is the train's motion after the tracks rejoin. And if the engineer's flipping the switch were used to represent those paths, *it* would fill the representative role that the train's departure would *better* fill, thereby linguistically screening off the train's departure and other events that are better representatives of the relevant paths. The problem is not that there is something wrong with screening off simpliciter—that happens all the time. The problem, rather, is that many of the screened off candidates *do much better at representing the causal paths to the arrival* than the flip. But this does not mean that the flip is not causally relevant to the arrival. It also does not mean that the flip is a bad representative of causal paths through it that end in events that occur while the train is on the right-hand track. Indeed, it's plausible that the flip will be quite a good representative of *those* causal paths.

This sort of explanation does not depend on there being a total ordering of the events that are causally relevant to e, in terms of their fitness for being counted as a cause of e. Indeed, I doubt that events *can* be ordered in this way. Is the spark a better or worse candidate to count as a cause of the fire than the presence of oxygen? Is my dropping the glass a better or worse candidate to count as a cause of its shattering than its fragility? Even relative to particular contexts such questions do not always have good answers. So there are many pairs of events that are both causally relevant to some

event e but not comparable with respect to their fitness for being counted as among the causes of e. What the explanation does rely on is the claim that *given a particular causal path* some events are better representatives of it than others. When we ask, of the causal paths that run through the flipping of the switch to the arrival of the train, whether *some* events better represent them than the flipping of the switch, the answer is clearly 'yes': after all, there is the departure, the train's movement after the tracks rejoin, the entire course of the train before it arrives, and so on.

What goes into making an event a good representative of a causal path is an important and difficult question. And some philosophers of causation have arguably already made significant progress on it, under a different mode of presentation. That is, when they proffer analyses that do better than "broad and non-discriminatory" analyses at matching our intuitions about what can count as a cause of what, they think they have been doing metaphysics. But the philosopher with a broad theory of causation can reconstrue such work as providing resources that help her say which events count as good representatives of a causal path. Because conversational context helps determine which events are good representatives of a causal path, *every* metaphysical theory of causation needs a 'good representative' theory to mediate between it and our ordinary language judgments. The question is just how much work *that* theory should do, and how much work the *metaphysics* should do. The fact that a metaphysically broad theory of causation needs a more ambitious theory of good representatives than a metaphysically 'narrow' theory does not obviously cut against the broad theory. So the advocate of a broad metaphysics can simply put the techniques of a narrower 'metaphysics' of causation to work in her theory of good representatives.

But USE GOOD REPRESENTATIVES can also explain our judgments about important cases *without* this sort of help from the theory of good representatives. To see this consider a version of Hartry Field's bomb case (YABLO 2004, 119). (The important aspects of the case are represented in FIGURE 4.)

BOMB

> Billy plants a bomb in a room. Suzy comes into the room, notices the bomb, and flees. Suzy later has a checkup and is found to be in perfect health.

Billy's planting the bomb is a cause of Suzy's fleeing, and Suzy's fleeing is a cause of her perfect health the next day. But Billy's planting the bomb in no context felicitously counts as a cause of Suzy's perfect health. We can explain this, in a metaphysically neutral way, by appealing to USE GOOD REPRESENTATIVES. To say that the bomb *is* a cause of Suzy's perfect health the next day is again to screen off certain potential

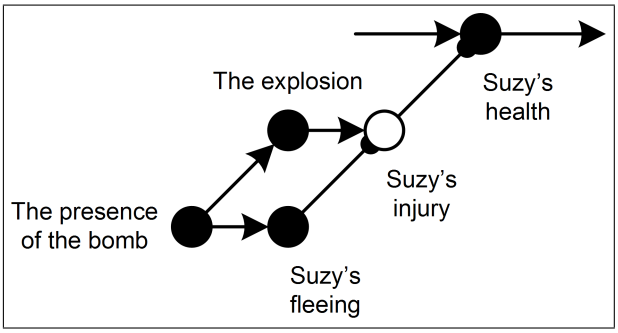


FIGURE 4

representatives of some causal paths to Suzy's perfect health. Among the screened off representatives is her fleeing the room. But her fleeing the room clearly does better at representing the causal paths through her fleeing the room to her subsequent health than Billy's planting the bomb would. After all, Billy's planting the bomb helped put her health in danger. So her fleeing the room can count as a cause of her health, but Billy's planting the bomb cannot.

The BOMB case is particularly interesting because it is one place where intuitions about causal relevance apparently diverge. Hall uses examples like BOMB to argue that if counterfactual dependence between distinct events sufficed for causation, then causation would not be transitive: Suzy's fleeing counterfactually depends on the presence of the bomb, and Suzy's health counterfactually depends on her fleeing, but the presence of the bomb was not a cause of her health (2004, 246–248). But whether the bomb can in any context count as *a* cause of Suzy's health is beside the point. What matters, if we debate the friend of causal relevance on her own terms, is whether the bomb was *causally relevant* to Suzy's health. Hall thinks it is not (p.c.); Lewis thinks it is.[16] I think

16. Perhaps you suspect that Lewis is in the grips of a theory. Perhaps he is; but he does have an independent rationale. In particular, it is because of the bomb's presence that Suzy's health is caused in one way rather than another. So the bomb's presence causally influences the causal history of Suzy's health, and is thus part of the causal history of Suzy's health (paraphrasing LEWIS 2000, 97–98). Moreover, Lewis sometimes suggests that causal histories *just are* "whole" causal explanations (1986a, 218–219). If we were trying to give "the biggest chunk of explanatory information that is free from error" (218) about Suzy's health, we would have to include her fleeing. But it would be obscure *why* we had included her fleeing—why her fleeing was *explanatorily relevant*—if we did not include the bomb's presence. So we must include it. By Lewis's lights, the causal history of Suzy's health must

it is not too surprising to find that there is not complete non-collusive agreement about what (in certain cases) is causally relevant to what. But note that in a debate between Hall and Lewis over whether the bomb is causally relevant to Suzy's health, rejection of (30) will not be probative.

(30) #Billy's planting the bomb was a cause of Suzy's later perfect health.

This is because, as have already seen, USE GOOD REPRESENTATIVES is powerful enough to explain *on its own* why we would never say (30). For this reason our judgments about (30) and similar sentences are compatible with a wider range of metaphysics of causation than we might have thought.

Lewis's line on BOMB is not vindicated by the fact that he can explain why we would never count the bomb's presence as a cause of Suzy's health. But it does show that Lewis can agree with common sense that the bomb in no context counts as a cause of Suzy's health, and that he has a way to explain why this is the case even if (as he claims) the bomb is causally relevant to Suzy's health. So in response to a philosopher who claims that BOMB is a counterexample to causal transitivity simpliciter (YABLO 2004, 119), or that such cases are counterexamples to the conjunction of transitivity and the sufficiency of counterfactual dependence (HALL 2004, 246–248), Lewis could observe that the planting of the bomb is in no ordinary context a good representative of the causal paths through it to Suzy's health. Indeed, USE GOOD REPRESENTATIVES affords a metaphysically neutral way for *any* theorist of causation to explain away our linguistic judgments about cases like BOMB.

3.3. Context sensitivity and the methodology of metaphysics

We started by looking for an illuminating story about 'causation itself'—a metaphysical natural kind the character of which we took to be independent of us, independent of the contingencies of our causal talk, and independent of our concept or concepts of causation. I argued that our ordinary causal claims are influenced by conversational context in significant ways, and so we turned to a not quite ordinary term—'causal relevance'—in the hope that *it* denotes the fundamental causal relation in a context free way. But then we found that in certain cases philosophers disagree about what even *counts* as causally relevant to what—let alone which is the true *theory* of causal relevance. And I argued, further, that in some cases judgments about ordinary causal

include it, too.

talk do not indicate what counts as causally relevant to what, because we can explain those judgments in principled but metaphysically neutral ways. In effect, I showed that once we begin to flesh out the theory that interfaces between our metaphysics of causation and our causal talk, there is a strong possibility that our causal talk will grossly underdetermine the metaphysics of causal relevance.[17]

We shouldn't be surprised to arrive at this point. Given that we need complementary theories—one metaphysical, one linguistic—to get substantive predictions about our causal talk, it would be fortuitous if our causal talk came anywhere close to uniquely determining the metaphysics of causation. In some respects this underdetermination is liberating, because we no longer have to worry about pesky cases like BOMB. But it also raises troubling methodological questions: if linguistic judgments dramatically underdetermine our metaphysics of causation, exactly what will help determine it? This question may have a good answer, but I am not sure what it is.

For the time being I think it's reasonable to put such skeptical worries to the side. Having a better sense of what work can be done by our linguistic theory gives us a better sense of where to look for examples that are genuinely probative for metaphysics. That said, whether a linguistic judgment is probative for metaphysics depends on the specific ways in which we explain the context sensitivity of causal talk. So we cannot make much real progress on the metaphysics of causal relevance without better understanding causal talk. It's also worth noting that some of the overlooked features of causal talk raise new questions for metaphysics. For example, I think we should consider the possibility that causes are not particular events (or facts, or whatever) but rather are causal paths themselves. These are roughly instrumental reasons to be interested in the context sensitivity of causal talk: understanding it may help us hone the knife with which we try to carve causal reality at its joints.

But I want to warn against the thought that the study of causal talk is *just* instrumentally important—that we would do well to ignore it if we could find a way to theorize about causal relations without the intermediary of judgments about causal claims. For an analogy, consider your initial, unarticulated philosophical curiosity about the nature of friendship. The property of being a friendly acquaintance of $\mathcal{P}$ is broad and non-discriminatory, instantiated at least by anyone who in some conversational context counts as a friend of $\mathcal{P}$. We can know quite a lot about this relation without knowing

17. This underdetermination might not worry you, if your only aim was to codify some necessary conditions for an event's counting as a cause in some context. But most metaphysicians take their projects to be more ambitious than this.

anything about the ways in which 'is a friend of' is sensitive to conversational context. But saying what it is for two people to be friendly acquaintances obviously does little to address our curiosity about *friendship*. Ignoring context sensitivity makes our task easier—it's clearly *easier* to think about friendly-acquaintance-ship than it is to think about friendship—but ease of theorizing does not warrant such a change in subject. Any philosophically respectable course here will have to deal with or work around the context sensitivity of 'is a friend of.'

Similarly, we cannot ignore the context sensitivity of causal talk without neglecting much that is of philosophical interest. This is because our unarticulated curiosity about causation is in part a curiosity about causal *thinking*, which is crucial to folk psychology, moral judgment, scientific reasoning, and a host of other philosophically rich topics. It's impossible to cleanly excise ordinary causal talk from ordinary causal thinking, so to study one is to study the other. And we should *welcome* this connection: to take just one example, progress on the theory of good representatives would likely help us better understand the connections between normative and causal judgments. I acknowledge that it's rarely cheering to think that we cannot make progress on a family of philosophical problems without better understanding some related conversational context sensitivity. But we should not assume that we can satisfy our initial curiosity about causation without such an understanding.

References

Bach, Kent. 1999. "The Myth of Conventional Implicature." *Linguistics and Philosophy*, vol. 22: 327–366.

Barwise, John & John Perry. 1981. "Semantic Innocence and Uncompromising Situations." *Midwest Studies in Philosophy*, vol. 6: 387–403.

—. 1983. *Situations and Attitudes*. MIT Press, Cambridge.

Beaver, David I. 2001. *Presupposition and Assertion in Dynamic Semantics*. CSLI Publications, Stanford.

Bell, J. M. 1973. "What is Referential Opacity?" *Journal of Philosophical Logic*, vol. 2: 155–180.

Bennett, Jonathan. 1988. *Events and Their Names*. Hackett Publishing Company, Indianapolis.

—. 1995. *The Act Itself*. Oxford University Press, Oxford.

Burge, Tyler. 1973. "Reference and Proper Names." *Journal of Philosophy*, vol. 70: 425–439.

Chierchia, Gennaro & Sally McConnell-Ginet. 2000. *Meaning and Grammar: An Introduction to Semantics*. MIT Press, Cambridge, second edn.

Cinque, Guglielmo. 1999. *Adverbs and Functional Heads: A Cross-Linguistic Perspective*. Oxford University Press, Oxford.

Clark, Herbert H. 1992. *Arenas of Language Use*. The University of Chicago Press, Chicago.

Clark, Herbert H. & Thomas B. Carlson. 1981. "Context for Comprehension." In Clark (1992), 60–77.

Clark, Herbert H. & Catherine R. Marshall. 1981. "Definite Reference and Mutual Knowledge." In Clark (1992), 9–59.

Clark, Herbert H., Robert Schreuder & Samuel Buttrick. 1983. "Common Ground and the Understanding of Demonstrative Reference." In Clark (1992), 78–99.

Coates, Jennifer. 1983. *The Semantics of the Modal Auxiliaries*. Croom Helm, London.

Collins, John, Ned Hall & L. A. Paul, editors. 2004. *Causation and Counterfactuals*. MIT Press, Cambridge.

Cooper, Robin. 1983. *Quantification and Syntactic Theory*. Reidel, Dordrecht.

Cresswell, Max. 1973. *Logics and Languages*. Methuen, London.

DeRose, Keith. 1991. "Epistemic Possibilities." *Philosophical Review*, vol. 100 (4): 581–605.

Donnellan, Keith. 1966. "Reference and Definite Descriptions." *Philosophical Review*, vol. 75: 281–304.

Drubig, Hans Bernhard. 2001. "On the Syntactic Form of Epistemic Modality." Ms., University of Tübingen, URL `http://www.sfb441.uni-tuebingen.de/b2/papers/DrubigModality.pdf`.

Earman, John. 1992. *Bayes or Bust?: A Critical Examination of Bayesian Confirmation Theory*. MIT Press, Cambridge.

Edgington, Dorothy. 1995. "On Conditionals." *Mind*, vol. 104: 235–329.

Egan, Andy. 2005. "Epistemic Modals, Relativism, and Assertion." Ms., University of Michigan, URL http://www.geocities.com/eganamit/papers.html.

Egan, Andy, John Hawthorne & Brian Weatherson. 2005. "Epistemic Modals in Context." In *Contextualism in Philosophy*, Gerhard Preyer & Georg Peter, editors. Oxford University Press, Oxford.

Elugardo, Reinaldo. 2002. "The Predicate View of Proper Names." In *Logical Form and Language*, Gerhard Preyer & Georg Peter, editors, 467–503. Oxford University Press, Oxford.

von Fintel, Kai. 2003. "Epistemic Modals and Conditionals Revisited." Ms., Massachusetts Institute of Technology, URL http://web.mit.edu/fintel/www/umass-handout.pdf.

—. 2004. "Would You Believe It? The King of France is Back! Presuppositions and Truth-Value Intuitions." In *Descriptions and Beyond: An Interdisciplinary Collection of Essays on Definite and Indefinite Descriptions and Other Related Phenomena*, Marga Reimer & Anne Bezuidenhout, editors, 315–341. Oxford University Press, Oxford.

von Fintel, Kai & Anthony S. Gillies. 2005. "*Might* Made Right." Ms., Massachusetts Institute of Technology and University of Michigan, Ann Arbor, URL http://mit.edu/fintel/www/might.pdf.

von Fintel, Kai & Sabine Iatridou. 2003. "Epistemic Containment." *Linguistic Inquiry*, vol. 34 (2): 173–198.

Gaifman, Haim. 1986. "A Theory of Higher Order Probabilities." In *Proceedings of TARK I*, Joseph Y. Halpern, editor.

Garrett, Brian Jonathan. 1998. "Pluralism, Causation and Overdetermination." *Synthese*, vol. 116 (3): 355–378.

Geurts, Bart. 1997. "Good News About the Description Theory of Names." *Journal of Semantics*, vol. 14: 319–348.

—. 1998. "Presuppositions and Anaphors in Attitude Contexts." *Linguistics and Philosophy*, vol. 21: 545–601.

—. 2002. "Bad News for Anyone? A Reply to Abbott." *Journal of Semantics*, vol. 19: 203–207.

Glanzberg, Michael & Susanna Siegel. 2006. "Presupposition and Policing in Complex Demonstratives." *Noûs*, vol. 40 (1): 1–42.

Goldberg, Jeffrey, László Kálmán & Zoltán Gendler Szabó. 1990. "Internal and External Presuppositions." In *Reader for the Nijmegen Workshop on Presupposition, Lexical Meaning and Discourse Processes*, Rob van der Sandt, editor. University of Nijmegen.

Graff, Delia. 2001. "Descriptions as Predicates." *Philosophical Studies*, vol. 102: 1–42.

Grice, Paul. 1957. "Meaning." In Grice (1989), 213–223.

—. 1961. "The Causal Theory of Perception." *Proceedings of the Aristotelian Society*, 121–152.

—. 1987. "Logic and Conversation." In Grice (1989).

—. 1989. *Studies in the Way of Words*. Harvard University Press, Cambridge.

Hacking, Ian. 1967. "Possibility." *Philosophical Review*, vol. 76 (2): 143–168.

Hall, Ned. 2000. "Causation and the Price of Transitivity." In Collins et al. (2004), 181–203.

—. 2004. "Two Concepts of Causation." In Collins et al. (2004), 225–276.

Halliday, Michael A. K. 1970. "Functional Diversity in Language as Seen from a Consideration of Mood and Modality in English." *Foundations of Language*, vol. 4: 225–242.

Heck, Richard. 1995. "The Sense of Communication." *Mind*, vol. 104: 79–106.

Heim, Irene. 1982. *The Semantics for Definite and Indefinite Noun Phrases*. Ph.D. thesis, University of Massachusetts, Amherst.

—. 1983. "On the Projection Problem for Presuppositions." In Portner & Partee (2002), 249–260.

—. 1992. "Presupposition Projection and the Semantics of Attitude Verbs." *Journal of Semantics*, vol. 9: 183–221.

Heim, Irene & Angelika Kratzer. 1998. *Semantics in Generative Grammar*. Blackwell Publishers, Ltd., Malden, MA.

Hornsby, Jennifer. 1976. "Proper Names: A Defense of Burge." *Philosophical Studies*, vol. 30: 227–234.

—. 1977. "Singular Terms in Contexts of Propositional Attitude." *Mind*, vol. 86 (341): 31–48.

Huddleston, Rodney & Geoffrey K. Pullum, editors. 2002. *The Cambridge Grammar of the English Language*. Cambridge University Press, Cambridge.

Huitink, Janneke & Jennifer Spenader. 2004. "Cancelation Resistant PCIs." URL `http://www.phil.kun.nl/tfl/janneke/huitink-spenader.pdf`.

Jeffrey, Richard C. 1968. "Probable Knowledge." In *Probability and the Art of Judgment*, 30–43. Cambridge University Press, Cambridge.

—. 1983. *The Logic of Decision*. University of Chicago Press, Chicago, second edn.

—. 1986. "Probabilism and Induction." *Topoi*, vol. 5: 51–58.

Joyce, James M. 1998. "A Nonpragmatic Vindication of Probabilism." *Philosophy of Science*, vol. 65 (4): 575–603.

Karttunen, Lauri. 1973a. "The Last Word." Ms., University of Texas, Austin.

—. 1973b. "Presuppositions of Compound Sentences." *Linguistic Inquiry*, vol. 4: 167–193.

—. 1974. "Presupposition and Linguistic Context." *Theoretical Linguistics*, vol. 1: 181–194.

—. 1977. "Syntax and Semantics of Questions." *Linguistics and Philosophy*, vol. 1: 3–44.

Kim, Jaegwon. 1989. "Mechanism, Purpose, and Explanatory Exclusion." In *Supervenience and Mind: Selected Philosophical Essays*, 237–264. Cambridge University Press, Cambridge.

King, Jeffrey C. 2001. *Complex Demonstratives: A Quantificational Account*. MIT Press, Cambridge.

Kratzer, Angelika. 1977. "What *Must* and *Can* Must and Can Mean." *Linguistics and Philosophy*, vol. 1: 337–355.

—. 1981. "The Notional Category of Modality." In *Words, Worlds, and Contexts: New Approaches in Word Semantics*, Hans Jurgen Eikmeyer & Hannes Rieser, editors, 38–74. W. de Gruyter, Berlin.

—. 1991. "Modality." In von Stechow & Wunderlich (1991), 639–650.

Krifka, Manfred. 2001. "Quantifying into Question Acts." *Natural Language Semantics*, vol. 9: 1–40.

—. 2004. "Semantics Below and Above Speech Acts." Ms., Humboldt Universität, URL `http://amor.rz.hu-berlin.de/~h2816i3x/StanfordLecture2004.pdf`.

Kripke, Saul. 1980. *Naming and Necessity*. Harvard University Press, Cambridge.

Larson, Richard & Gabriel Segal. 1995. *Knowledge of Meaning: An Introduction to Semantic Theory*. MIT Press, Cambridge.

Lewis, David K. 1969. *Convention: A Philosophical Study*. Harvard University Press, Cambridge.

—. 1970. "General Semantics." In Lewis (1983).

—. 1973. "Causation." In Lewis (1986b), 159–213.

—. 1976a. "Probabilities of Conditionals and Conditional Probabilities." In Lewis (1986b), 133–56. With postscript.

—. 1976b. "Survival and Identity." In Lewis (1983), 55–77. With postscript.

—. 1979. "Scorekeeping in a Language Game." In Lewis (1983), 233–249.

—. 1980. "A Subjectivist's Guide to Objective Chance." In Lewis (1986b), 83–132. With postscript.

—. 1983. *Philosophical Papers*, vol. 1. Oxford University Press, Oxford.

—. 1986a. "Causal Explanation." In Lewis (1986b), 214–240.

—. 1986b. *Philosophical Papers*, vol. 2. Oxford University Press, Oxford.

—. 1996. "Elusive Knowledge." In *Papers in Metaphysics and Epistemology*. Cambridge University Press, Cambridge.

—. 2000. "Causation as Influence." In Collins et al. (2004), 75–106.

MacFarlane, John. 2003. "Epistemic Modalities and Relative Truth." Ms., University of California, Berkeley, URL `http://philosophy.berkeley.edu/macfarlane/work.shtml`.

Mackie, J. L. 1980. *The Cement of the Universe: A Study of Causation*. Oxford University Press, Oxford, second edn.

Malcolm, Norman. 1968. "The Conceivability of Mechanism." *Philosophical Review*, vol. 77 (1): 45–72.

McCawley, James D. 1970. "Where Do Noun Phrases Come From?" In *Readings in English Transformational Grammar*, R. A. Jacobs & P. S. Rosenbaum, editors. Ginn and Co., Waltham, MA.

Mellor, D. H. 1980a. "Consciousness and Degrees of Belief." In Mellor (1980b).

Mellor, D. H., editor. 1980b. *Prospects for Pragmatism: Essays in Memory of F. P. Ramsey*. Cambridge University Press, Cambridge.

Merricks, Trenton. 2001. *Objects and Persons*. Oxford University Press, Oxford.

Monderer, Dov & Dov Samet. 1989. "Approximating Common Knowledge with Common Beliefs." *Games and Economic Behavior*, vol. 1: 170–190.

Montague, Richard. 1973. "The Proper Treatment of Quantification in Ordinary English." In Portner & Partee (2002), 17–34.

Morris, Stephen & Hyun Song Shin. 1997. "Approximate Common Knowledge and Co-ordination: Recent Lessons from Game Theory." *Journal of Logic, Language, and Information*, vol. 6: 171–190.

Palmer, F. R. 2001. *Mood and Modality*. Cambridge University Press, Cambridge, second edn.

Pearl, Judea. 2000. *Causality: Models, Reasoning, and Inference*. Cambridge University Press, Cambridge.

Pereboom, Derk. 2002. "Robust Nonreductive Materialism." *Journal of Philosophy*, vol. 99: 499–531.

Perry, John. 1977. "Frege on Demonstratives." *Philosophical Review*, vol. 86: 474–97.

Portner, Paul & Barbara H. Partee, editors. 2002. *Formal Semantics: The Essential Readings*. Blackwell Publishers Ltd., Oxford.

Potts, Christopher. 2005. *The Logic of Conventional Implicatures*. Oxford University Press, Oxford.

Quine, W. V. O. 1953. "On What There Is." In *From a Logical Point of View*, 1–19. Harvard University Press, Cambridge, second edn.

Recanati, François. 1986. "Contextual Dependence and Definite Descriptions." *Proceedings of the Aristotelian Society*, vol. 87: 57–73.

—. 1993. *Direct Reference: From Language to Thought*. Blackwell Publishers, Ltd., Malden, MA.

—. 2000. *Oratio Obliqua, Oratio Recta: An Essay on Metarepresentation*. MIT Press, Cambridge.

—. 2001. "Open Quotation." *Mind*, vol. 110: 637–687.

Richard, Mark. 1990. *Propositional Attitudes: An Essay on Thoughts and How We Ascribe Them*. Cambridge University Press, Cambridge.

Russell, Bertrand. 1905. "On Denoting." In *Definite Descriptions: A Reader*, Gary Ostertag, editor, 35–49. MIT Press, Cambridge.

—. 1919. "Descriptions." In *Readings in the Philosophy of Language*, Peter Ludlow, editor, 323–334. MIT Press, Cambridge.

Sadock, Jerrold. 1978. "On Testing for Conversational Implicature." In *Pragmatics: A Reader*, Steven Davis, editor, 365–376. Oxford University Press, Oxford.

Sahlin, Nils-Eric. 1994. "On Higher Order Beliefs." In *Philosophy of Probability*, J. P. Dubucs, editor, 13–34. Kluwer, Boston.

Salmon, Nathan. 1986. *Frege's Puzzle*. MIT Press, Cambridge.

Saul, Jennifer. 1998. "The Pragmatics of Attitude Ascription." *Philosophical Studies*, vol. 92.

Schiffer, Stephen R. 1972. *Meaning*. Oxford University Press, Oxford.

Searle, John. 1969. *Speech Acts: An Essay in the Philosophy of Language*. Cambridge University Press, Cambridge.

Searle, John & Daniel Vanderveken. 1985. *Foundations of Illocutionary Logic*. Cambridge University Press, Cambridge.

Shanon, Benny. 1976. "On the Two Kinds of Presuppositions in Natural Language." *Foundations of Language*, vol. 14: 247–249.

Shoemaker, Sydney. 2001. "Realization and Mental Causation." In *Physicalism and its Discontents*, Carl Gillett & Barry Loewer, editors, 74–98. Cambridge University Press, Cambridge.

Skyrms, Bryan. 1977. "Resiliency, Propensities, and Causal Necessity." *Journal of Philosophy*, vol. 74: 704–713.

—. 1980a. *Causal Necessity*. Yale University Press, New Haven.

—. 1980b. "Higher Order Degrees of Belief." In Mellor (1980b).

Soames, Scott. 2002. *Beyond Rigidity: The Unfinished Semantic Agenda of Naming and Necessity*. Oxford University Press, Oxford.

—. 2005. "Naming and Asserting." In *Semantics versus Pragmatics*, Zoltán Gendler Szabó, editor, 356–382. Oxford University Press, Oxford.

Stalnaker, Robert C. 1978. "Assertion." In Stalnaker (1999), 78–95.

—. 1981. "Indexical Belief." In Stalnaker (1999), 130–149.

—. 1986. "Possible Worlds and Situations." *Journal of Philosophical Logic*, vol. 15: 109–123.

—. 1988. "Belief Attribution and Context." In Stalnaker (1999), 150–166.

—. 1999. *Context and Content*. Oxford University Press, Oxford.

—. 2002. "Common Ground." *Linguistics and Philosophy*, vol. 25: 701–721.

—. 2005. "On What There Isn't (But Might Have Been)." Ms., Dept. of Linguistics and Philosophy, MIT.

von Stechow, Arnim & Dieter Wunderlich, editors. 1991. *Semantics: An International Handbook of Contemporary Research*. W. de Gruyter, Berlin.

Swanson, Eric. 2005. "Pronouns and Complex Demonstratives." Ms., Department of Linguistics and Philosophy, MIT.

Szabó, Zoltán Gendler. 2000. "Compositionality as Supervenience." *Linguistics and Philosophy*, vol. 23: 475–505.

Teller, Paul. 1972. "Epistemic Possibility." *Philosophia*, vol. 2: 302–320.

Thau, Michael. 2002. *Consciousness and Cognition*. Oxford University Press, Oxford.

Unger, Peter. 1977. "The Uniqueness in Causation." *American Philosophical Quarterly*, vol. 14: 177–188.

—. 1984. *Philosophical Relativity*. University of Minnesota Press, Minneapolis.

Westmoreland, Robert R. 1998. *Information and Intonation in Natural Language Modality*. Ph.D. thesis, Indiana University.

Wilson, Jessica. 1999. "How Superduper does a Physicalist Supervenience Need to Be?" *Philosophical Quarterly*, vol. 49: 33–52.

Yablo, Stephen. 1987. "Identity, Essence, and Indiscernibility." *Journal of Philosophy*, vol. 84: 293–314.

—. 1992. "Mental Causation." *Philosophical Review*, vol. 101: 245–280.

—. 2004. "Advertisement for a Sketch of an Outline of a Prototheory of Causation." In Collins et al. (2004), 119–137.

Yalcin, Seth. 2005. "Epistemic Modals." MIT Working Papers in Linguistics, vol. 51. URL `http://mit.edu/yalcin/www/epmodal-dec05.pdf`.

Zeevat, Henk. 1994. "A Speculation about Certain Triggers." In *The Proceedings of the IBM/Journal of Semantics Conference on Focus*, Rob van der Sandt & Peter Bosch, editors, vol. 3, 669–676. IBM, Heidelberg.